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OF

# NATURAL HISTORY.

EDITED BY

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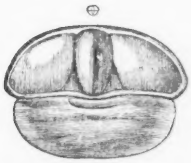

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# CONTENTS OF VOLUME VII.

## REVIEWS AND BOOK NOTICES.

## BOTANY.



## ZOOLOGY.

## GEOLOGY.



## ANTHROPOLOGY.



## MICROSCOPY.

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## ERRATA.

Page 115, line 9, for Maynard *read* May. Page 177, line 16, for females (workers and queens) *read* drones, and in line 17, for drones *read* females (workers and queens). Page 228, lines 3 and 10 from bottom, for Bagshot *read* Bagehot. Page 530, Fig. 138 is not taken from Harris' work, but from Riley's Report on Injurious Insects of Missouri. Page 544, Fig. 147 is upside down. Page 567, line 1, for improved *read* unproved.

## LIST OF PLATES.



## LIST OF WOOD-CUTS.

THE

# AMERICAN NATURALIST.

Vol. VII.—JANUARY, 1873.—No. 1.

## NOTES ON THE RIGHT AND SPERM WHALES.

BY PROF. N. S. SHALER.

The following notes on the habits of the right whale were taken down in a conversation with Captain John Pease of Edgartown, an old whaler, whose powers of observation as well as of accurate and clear statement I have rarely known equalled. As far as possible these statements have been collated with those of other experienced whalers.

All of the south latitude right whales are without calves up to July 1st; the females are found in the bays about this time. The calves all come at once, it being but two or three days between the bearing of the first and last calves. None are found with the herd up to the 1st of July and every female has her calf by the 3d or 4th of the month.

The right and humpback whales are very fond of their young, taking no care of themselves in their efforts to save it; the sperm whales, on the other hand, are quite without affection as far as can be determined by their behavior.

Sperm whales have leaders of the herd which they follow with a certain obstinacy; these leaders seem to give the alarm to the others. No such subordination can be observed among right whales. Sperm whales, as is well known, have the males very much larger than the females, while the reverse is the case among the right whales. This is interesting in connection with the fact that the male sperm whales struggle furiously together, while the

males of the right whales seem to have no conflicts with each other. Captain Pease had seen males struggling with each other and often found their bodies scarred with the imprints of the rival's teeth; the scars showing their origin very distinctly by their form — the distance apart of the wounds answering to the intervals of the teeth. The great superiority in the size of the males among the sperm whales is just what would be expected in a species where the males struggled in the combats of rivals. The gain in size under the influence of these conflicts of the males is generally limited in land animals within pretty narrow bounds. There are probably no land animals where the male is double the weight of the female, yet the male sperm whale would seem to excel the female by more than this proportion. This extreme development of the males occurs also among the Otaridæ as well as among many groups of fishes, so it would seem as if there was some reason why the influences tending to limit size were less active in the sea than on the land. The reason for the greater freedom to acquire size in the sea is undoubtedly to be found in the less weight of bodies in that element, the effect of which is shown as well in the structures of man as in the structures of nature; the ship exceeds all vehicles for land transportation for the same reason and in something like the same proportion that marine animals, when size is the advantage, exceed terrestrial forms.

The conflicts between the males of sperm whales lead to great damage to the lower jaw; the evidence goes to show that at least two per cent. are crooked more or less, and one in several hundred very badly bent by these struggles. There are two specimens in the small museum at Nantucket which are singularly contorted; one of them is bent laterally into one turn of a spiral. Captain Pease tells me that he found one that was bent sideways at right angles to the proper position and firmly fixed there, seeming to be a permanency in this singular place. In fighting, the males rush at each other with open jaws and strike in passing. The great speed and power of these massive creatures must lead to the most serious results from these collisions. Capt. Pease found a sperm whale nearly dead on the water with the lower jaw hanging by a single band of ligament a few inches through. The creature was being devoured by sharks and crustaceans, but the wrench which had crippled this whale must have come from one of his kind.

Captain Pease has several times seen the killer attack right and humpback whales; they strike for the tongue if possible. They often jump many feet from the water and fall upon him. Many individuals, fifty or more, join in this attack. They tear out large pieces from the blubber, food being evidently the object of their attack. Their great activity makes the whale helpless against them, though he will struggle furiously before overborne. They sometimes drag down the whale after it has been killed by the whalemen.

The Captain was quite sure that the chief article of food of the sperm whale is squid, as they vomit large quantities of them in their death agonies; he thinks that the whales take them by swimming with the mouth so wide open that the lower jaw stands at nearly right angles to the upper. Squid, he thinks, will grasp at the jaw as the whale passes among them and are cut in fragments by the sudden closure of the jaws. He says that the jaw is closed with prodigious force and suddenness so that when out of water the noise can be heard for two or three miles, and is even noticeable under water. He stoutly maintains that he has seen fragments of squid, where the whales had cut them in two, exposing the cavity of the body, which was as large over as the head of a forty gallon cask. In one case he saw the head of a squid which he believes to have been as large as a sugar hogshead.

The Captain is convinced that the right whale has a trace of hair within the skin. He says that when the skin is fresh, if it be scraped with a knife so as to remove the superficial parts, there will then be seen a trace of hair in the inner section. This point is worthy of attention from those naturalists who have opportunities for such work. It is evident that if the whale is the descendant of some land mammal form it would be likely to preserve a trace of the hairy covering. In this connection it is interesting to note that, in the museum at Nantucket, there is a tooth of a sperm whale with two fangs after the fashion of an ordinary mammalian canine. The specimen was taken many years ago, but with it is the statement that the other teeth of the whale were of the same fashion. This clearly looks like a reversion to some higher mammalian form of dentition.

Captain Pease thinks that right whales attain very nearly their adult size in three years, there being about three distinct sizes found at one time in the sea. He thinks, however, that they may continue to grow very slowly for some years longer, the ultimate

size depending a good deal upon the haunt of the whale; some regions having larger specimens than others. If the whales are descendants of our marine carnivora we should expect them to preserve something like the same growth rates, for this feature seems to be tolerably permanent in any group of related animals. The rate of growth, deducible from the observations of the practical students of the whale, coincides pretty closely with what we should be inclined to expect on the supposition that the cetacea were descended from some ancestor like the marine carnivora.

The great decline of the whale fishery in all countries seems likely to deprive us of the ill-used opportunities, which naturalists have long had, of making themselves acquainted with the habits of the greatest of the mammals. There are many questions which should be discussed and settled before the class of clear headed and observant whalemen has passed away; else we may remain for centuries without a competent knowledge of the ways of this, the greatest living monument of animal life.

## OUR POISONOUS PLANTS.

BY W. W. BAILEY.

The poisonous plants of our northern woods are not so numerous but that they may easily be learned. Of them certain members of the sumac family (*Anacardiaceæ*) have the most evil reputation. To this order belong the so-called dogwood (*Rhus venenata*), and the poison-ivy (*Rhus toxicodendron*). The dogwood must not be confounded with the beautiful *Cornus florida*, which unfortunately bears the same familiar name. This tree is perfectly innocent and is so highly ornamental that it would be a shame if through simple ignorance it should ever be cut down.

There are two varieties of *Rhus toxicodendron*, distinguished by slight differences in the leaves. When these are cut-lobed, the plant is *R. toxicodendron* (Fig. 1); when entire, it bears the name of *R. radicans*. Some authors have considered them distinct species, but there is no doubt they are merely modifications of one. Both forms are occasionally mistaken for the Virginian creeper (*Ampelopsis quinquefolia*, Fig. 2) to which the climbing forms bear

little resemblance. They may, however, be always distinguished from that graceful plant by the three leaflets on a stem, and by the mossy aggregation of roots by which they adhere to trees and rocks. The Virginian creeper, on the contrary, has *five* leaflets and is furnished with tendrils which expand into sucker-like disks to assist the plant in climbing. It turns a vivid crimson in autumn, and as it is seen climbing some evergreen or trailing over a stone wall is one of the chief ornaments of that season. The poison-ivy also colors beautifully, but I think much sooner, and the tints are different, bright yellow, orange, or mahogany. Many persons have been induced, by their own ignorance or the superficial knowledge of their friends, to avoid or even destroy the harmless woodbine, or else have suffered by a too free handling of its mischievous neighbor. I say neighbor, as the two are often found near together and are similar in their habits of growth. The poison-ivy is very common, and may even be seen embracing the fences or wrapping large trees with its snaky branches. It is said sometimes to invest trees so closely as to cause their death. However that may be, I have seen its foliage entirely replace that of some lofty elm, now dead, and dependent alone for its beauty upon the plant the growth of which it had assisted.

A more dangerous plant, yet one of the most beautiful trees which we meet in swamps, is the poison-dogwood (*Rhus venenata* Fig. 3). It has from seven to thirteen leaflets on a common stalk, an odd one terminating the series. Its autumn coloring is magnificent, passing from green through a bright yellow, to crimson and scarlet, the midrib remaining in each case an intense red. Thoreau says, somewhere, that the plant appears to "blush for its sins." With its smooth gray bark and pinnate foliage it is conspicuous always, and when once known is easily remembered, but the desired information is often the result of a sad experience. Painful swellings, inflammation, and intense itchings are to many the result of contact with it, or even with the less noxious *Rhus toxicodendron*. Some persons are even affected by passing near, while others may handle it with absolute impunity. It is said, however, that even the chosen few are not always exempt from its influence, a profuse perspiration or some unusual condition of the system rendering those susceptible who usually have no cause to fear. I have myself often squeezed the leaves in my hands, and never avoid the tree when it lies in my way,

Fig. 1.

Poison Ivy (Rhus toxicodendron).

Fig. 2.

Virginia Creeper (Ampelopsis quinquefolia).

and I have as yet experienced no consequent suffering. The poisonous property of these plants appears to reside in the resinous juice, and may be removed by boiling and evaporation. Upon exposure to the air the juice blackens and forms an indelible ink.

The *Ranunculaceæ*, or crowfoot family, form a very suspicious order of plants; those which are not absolutely poisonous having generally an acrid or bitter juice. *Ranunculus acris* is especially caustic, and when fresh is avoided by cattle. Drying appears to

Fig. 3.

Poison Dogwood (Rhus venenata).

remove the poison. This is the tallest of our buttercups, with leaves "three divided; *the divisions all sessile* and three cleft or parted, their segments cut into lanceolate or linear crowded lobes." When taken internally some of the buttercups will produce dangerous symptoms, but this is an accident not very liable to happen, as their blistering tendency would cause them to be rejected without swallowing.

Fatal mistakes have occurred when persons have eaten the root of the monkshood (*Aconitum Napellus*) in early spring ere the

leaves served to distinguish it, thinking it to be horseradish. It is an introduced plant and will only be met with in cultivation, or in old gardens or waste places, and it is so generally known to all, that I will not delay to describe it. I will mention, however, a peculiar tingling sensation which it produces when applied to the tongue, an effect of some duration. The anemones, the larkspurs (*Delphinium*), and the bane-berries (*Actæa*) all contain in greater or less degree an active principle which becomes dissipated upon drying as in the case of the buttercups. Even the pretty roots of the common gold-thread (*Coptis trifolia*) are intensely bitter, and are sometimes used as a cure for children afflicted with diseases of the mouth. As a rule it is well to be cautious in our treatment of any plant the characters of which indicate that it belongs to the *Ranunculaceæ*.

The parsley family (*Umbelliferæ*) may be recognized by the small, generally white or yellow flowers, disposed in spreading umbels, with mostly compound leaves, often very delicately dissected, as in the common carrot (*Daucus carota*). The flowers and leaves of this plant, or of the parsnip or parsley, will serve as types of the whole order, to which belong many of our most noxious plants as well as wholesome vegetables. The species, owing to their similarity and the minuteness of the inflorescence, are difficult to distinguish and in consequence it can not be certainly affirmed how many are injurious. They are determined mostly from the seeds and flowers.

This, like the last, is a suspicious order, the more so, perhaps, from the fact of its containing certain edible members, for which their noxious relatives may be mistaken. Accidents are therefore of quite frequent occurrence, especially among children. Our native water-hemlock (*Cicuta maculata*, Fig. 4) is very poisonous. It is said that "a drachm of the fresh root has killed a boy in an hour and a half!" The plant is far too common for safety, and is found in swamps and wet places, even within the limits of our cities. It is a tall, rank herb, the smooth stems streaked with purple, the flowers white, and the veins of the compound leaves terminating in the notches. Still more to be avoided is the introduced hemlock (*Conium maculatum*) which has a very similar habit and appearance. It has smooth, spotted stems, and an offensive mousy smell, which treatment with potash brings out more strongly. It is supposed to be the poison by which the ancients eliminated

their troublesome politicians, and if this were not a serious article I might perhaps grow facetious, and suggest its use at the present time. It is now employed to some extent in medicine. Its name "hemlock" is an unfortunate one, as it is shared with that most elegant spruce, the *Abies Canadensis*, and I have known nervous people to avoid the latter for the sins of its fearful namesake. It

Fig. 4.

Water Hemlock (Cicuta maculata).

is said that the well known celery which belongs to this family and grows wild in England is, in its native state, dangerous for food, and is only made palatable and innocuous by the process of bleaching to which it is subjected. The active principle can only be developed with free access of light.

The nightshade, like the parsley family, contains both edible and poisonous plants. The potato (*Solanum tuberosum*), the egg-plant

(*Solanum melongena*), the tomato (*Lycopersicum esculentum*), the strawberry tomato (*Physalis*), are well known esculent vegetables; but even with these, certain portions of the plant are often poisonous or narcotic, as in the case of the potato, where the berries and leaves are injurious. I once saw a boy in New Brunswick eating the large green potato berries, but to my mild remonstrance he replied that he had often done so before without any resulting trouble. It would therefore appear that if actually dangerous, the fruit may not be so to all constitutions.

It is doubtful whether the bright red berries of the bitter-sweet (*Solanum dulcamara*) are in any degree injurious, but so long as their innocence is not established, it is just as well to treat them with caution. The common nightshade (*Solanum nigrum*), often found about houses, is more certainly dangerous. Young children, unless prevented, are almost sure to eat the berries of the bitter-sweet, attracted by their brilliant and luscious appearance. The bright blue, showy flowers bear a striking resemblance to those of potato. The thorn-apple (*Datura stramonium*) always found growing in waste places may be known by its morning-glory-like flowers, white, shaded with violet, its large, spiny seed pods, and its most offensive odor. As with the potato, the bitter-sweet, and other members of the genus *Solanum*, the leaves are always found perforated by insects. The seeds are said to have been used by the Delphic priests to excite their mad ravings, which the Greeks understood as prophecies.

In the order Liliaceæ, we have the American white hellebore (*Veratrum viride*), the root of which is a deadly poison. The plant is known familiarly as Indian poke, and has coarse fibrous roots, and elegantly plaited leaves, which in early spring may be seen by the banks of streams, generally in company with the skunk-cabbage, from which, however, it is easily distinguishable. The latter throws up its curiously painted, shell-like spathe in early April or even in March, the flower preceding the leaves, while the hellebore blooms in the summer, and has a tall upright spike of greenish flowers, in no respect resembling those of its neighbor. The active principle contained, is the alkaloid veratria, used to some extent in medicine.

The jack-in-the-pulpit (*Arisæma triphyllum*) is found in similar localities and, although not strictly a poison, its root is very acrid and caustic, as the children with the average propensity for inves-

tigation have discovered to their cost. The disgusting odor of the skunk-cabbage (*Symplocarpus fœtidus*) must always preclude similar experiments. Both of these plants belong to the order Araceæ, of which the sweet flag (*Acorus calamus*) is also a member.

Certain of the fig-worts (*Scrophulariaceæ*) are narcotic poisons, but I know of none which need any special mention. The dogbanes (*Apocynaceæ*) belong to a poisonous family of which it is well to be careful, although, so far as I am aware, our two pretty species need not be avoided. They have a milky acrid juice, as do the Euphorbias to which the same remarks apply. In the (*Urticaceæ*) we have the hemp (*Cannabis sativa*) which, in the east, yields the well known drug called hasheesh. In our climate, I believe this poison is not developed. The nettles belong to the same family but it is unnecessary to point out the eminent propriety of handling these with gloves, as some of them are provided with stinging hairs. According to Scott, they are when young used as greens in Scotland and cultivated for that purpose. (Rob Roy, Chap. 8).

The Indian tobacco so much used by quacks, is *Lobelia inflata*, a common little plant in open fields, with light blue flowers and inflated pods. The blossoms are very much smaller than those of the cardinal flower (*Lobelia cardinalis*), but of the same general appearance. All the lobelias are poisonous, and are much too recklessly employed by those who have little knowledge of their power. It is said by Darlington that the quacks give the name of *high*-belia to the cardinal flower to distinguish it from *low*-belia. This gives some idea of the amount of their learning.

There are some others of our native plants which possess an acrid juice, but I think I have now mentioned all that should be known with the exception of certain fungi with which I am not familiar. Among the grasses, there is but one, the darnel (*Lolium temulentum*), that has the reputation of being noxious, and late investigations appear to throw much doubt upon previous statements in regard to it. The *Kalmia latifolia* in Ericaceæ has been said to poison cattle, but the assertion has not been proved.

In reviewing the plants now mentioned, we find three that are poisons to the touch, *Rhus venenata*, *Rhus toxicodendron* and the nettles (*Urticæ*). The following are narcotic irritants, *Veratrum*, *Aconitum*, *Cicuta*, *Conium*, *Datura*, *Atropa*, *Lobelia*. The butter-

cups are acrid and caustic, as are the *Araceæ*, while Coptis is simply bitter.

I have been able to offer but a sketch of our poisonous plants, and may have omitted to mention a few. I have been surprised in studying them to find how little appears to have been written about them except as regards their medicinal effects, and how this little is distributed in many different books. I cannot close this article without a renewed warning against the reckless use of herbs whose effects may be deleterious or even fatal.

## A GLIMPSE AT COLORADO AND ITS BIRDS.

BY C. E. AIKEN.

Early this morning, the 17th of October, as I was riding past Beaver Creek, a large and beautiful mountain stream that flows through portions of El Paso, Fremont and Puebla Counties, my attention was attracted by a great twittering among the feathered tribe in an enclosure on the creek bottom. As there seemed to be an unusually large congregation of species for this season of the year, I dismounted from my pony, and leaning upon the cottonwood rail-fence, I watched the birds for nearly an hour, noting the different varieties, and observing the actions of each.

Immediately in front of me was a low, dense, wild-plum thicket, overrun and interwoven with hop-vines, but at this season nearly stripped of its leaves; and it seemed this morning as though each fallen leaf had been replaced with a little feathered songster. At least a dozen species were represented; but the white-crowned sparrows were by far the most numerous, and the singing or twittering of these it was, that first drew my attention.

Beyond this thicket, a thrifty growth of cottonwood extended along the banks of the creek from right to left, from the midst of which the songs of numerous robins, and of one or two other birds, rang out as clear and joyous as in early springtime. Many of the trees had their trunks encased in wild grape or hop-vines, and most of them were bare of leaves; but occasionally a tree clothed in a bright yellow foliage relieved the monotony and beautified the view. A high, rocky, barren ridge that formed the west

wall of the creek cañon extended across the background. At my right hand was a small stubble-field in the midst of the tangled brush, and a little to the left a clump of scrubby oaks. Several small trees scattered through the foreground, with here and there a clump of differently tinted red, green or yellow bushes, completed the landscape. Imagine now the whole enlivened with birds and you have the entire picture.

On account of their bright plumage and boisterous actions, Woodhouse's jay and the magpie were most prominent; particularly the former, of which there were about a dozen individuals that kept flying in and out among the bushes before me. Occasionally one would fly up on to the limb of a tree, where it would pause but a moment to swallow the morsel of food it had brought, or to look about it, and then off it went with a wild, chattering note. The low oak bushes that are so abundant in the foothills are the chosen haunts of these birds, and they are never found at any great distance from them. A magpie in the cottonwood grove, espying me, came over directly to satisfy his curiosity, which, by the way, is a prominent feature in his character. He alighted on the top of a fence-stake within ten feet of me, and giving his beautiful, long, glossy tail a jerk, and ducking his head impertinently, he uttered a harsh, bold note of inquiry; but when I turned my head to obtain a better view of him, he was off in an instant.

Another noticeable bird was the arctic finch (*Pipilo arcticus*). These were to be seen everywhere, among the bushes, on the ground, or flying from one thicket to another and, from their abundance, form one of the characteristic birds of this section. At this season they are very quiet, and usually keep themselves concealed in the brush; but during the early part of the season, the males were seen on every hand, perched in the top of some bush, and singing the same song that we are accustomed to hear from our "chewink" at the East. Nearly all of them have already left for warmer regions, and a few days more will probably see the last of them here, until they return next April. I noticed one of these little fellows busily scratching on the ground beneath some bushes close by, and nearly buried among the dead leaves he had heaped up around himself. Becoming shortly aware of my presence, he straightened up, raised the feathers of his crown into a crest, and twitching his little head first one way and then another, he sur-

veyed me from head to foot; then, as though satisfied that all was not right, he hopped cautiously to the next clump of bushes, and then flying close along the ground, disappeared in the thicket.

A dove, that alighted near me, stretched up its neck, looked timidly at me an instant, and then flew away, and a Townsend's flycatcher that came down from the cedar-clad ridge behind me to quench its thirst, lingered about for a few moments and then, becoming frightened at some invisible thing, hastened back to its secluded retreat. A red-shafted flicker rapped industriously for awhile, on an old dead cottonwood, and then left for more productive fields. Hearing the low whistle of the cedar bird above me, I looked up and saw several of them flying over. These were the first I had seen for nearly a year. In response to my call a flock of Arkansas finches (*Chrysomitris psaltria*), that were flying past, settled among the topmost twigs of the thicket, and silently eyed several purple and house finches that occupied similar positions about them. These little beauties are the last to greet us in summer, and among the last to leave in autumn, which is quite unusual in our summer visitors; those coming last being generally the first to leave and *vice versa*. They did not become common here this season until the first of July, yet I noticed them last fall as late as the fifth of November. The males still wear their summer plumage, and appear at a short distance as bright as when they first arrived from the South.

From the cottonwood grove, I recognized the familiar notes of the song sparrow, and soon one of these appeared in the edge of the thicket near me, with a Lincoln's finch for a neighbor. A flock of tree sparrows just from the North, and a solitary chipping sparrow that had lingered a few days behind the rest of his tribe, were also among the occupants of the thicket. The Oregon snowbird too, and the more recently described *Junco annectens*, were each represented there by a single individual; and once I thought I saw a chat among its branches, but as I have not observed any of these birds for a month, I was probably mistaken. Then a flock of six or eight bluebirds (Sialia arctica), probably an old pair with their young, passed on their way southward, and three or four Brewer's blackbirds that seemed to have no destination in particular made a short halt near by. Then a flock of thirty or forty noisy, cawing, Maximillian's jays settled down on the stubble-field where they remained until one of their number, seeing me, gave a caw, when

with a great racket they all rose together like a flock of blackbirds and returned to their haunts among the cedars far up the cañon. For some time a pair of mallard ducks had been circling about as though looking for a place to alight, and finally they selected a bend in the creek just in front of me. Above the ridge beyond the creek, a turkey buzzard was floating listlessly in the morning sun, apparently without the least exertion on his part. I watched him carefully for several moments as he circled about, but could not detect the slightest motion in his wings.

One other bird I saw here to which is attached a good deal of interest, the white-necked crow (*Corvus cryptoleucus*). I have found these birds common along the base of the Rocky Mountains, from Cheyenne at the north, to Trinidad at the south; and from the Snowy Range, to a point thirty miles out on the plain, yet Mr. Ridgway writes me that these birds "are entirely out of their previously known range." I strongly suspect that this bird has been mistaken by naturalists, who have ornithologized in this section, for the common American raven (*Corvus carnivorus*), since it seems to me impossible that any one should remain here any length of time without seeing it; still the Western bluebird (*Sialia Mexicana*), and several other birds which are equally abundant here, are in the same predicament. The raven is said to be common in Colorado, but during a year spent in collecting in different parts of the territory, I have seen but a single pair!

## HARVEST MITES.

BY PROF. C. V. RILEY.

In the "American Entomologist" (vol. 1, no. 5) an account was given of the eight true insects, and of some other ringed animals or articulates, known to be parasitic on man. The insects are, the head-louse (*Pediculus humanus* Linn.), the body-louse (*Pediculus cervicalis* Linn.), the crab-louse (*Pediculus pubis* Linn.), the human bot-fly (*Œstrus hominis* Gmelin), the common flea (*Pulex irritans* Linn.), the chigoe (*Pulex penetrans* Linn.), the common bed-bug (*Acanthia lectularia* Linn.) and the big bed-bug (*Conorhinus sanguisuga* Le C.).

The only mite that is known to attack man, and whose appearance is at all familiar, is the itch mite (*Acarus scabiei* Linn.). We have, however, in the southwestern States, two other mites which cause great annoyance from harvest time till into October, to people who frequent the rank herbage and grass in our forest openings or along our rivers. Both of them are six-legged, reddish, microscopic specks, and both are popularly termed jiggers; but as this term is universally applied to the more dangerous *Pulex penetrans* (a true flea occurring in Central America but not in the United States), and as a European mite (*Leptus autumnalis*), having similar habits to ours, is there popularly called "harvest bug," we may apply to our species the term "harvest mites."

Before we can talk intelligently and definitely of anything that moves or has a being upon our earth, it must receive some scientific appellation. According to my friend, A. S. Packard, Jr., and from our present knowledge of the transformation of mites, we may very plausibly conclude that these six-legged forms are but the young of some eight-legged form such as Trombidium, to which belongs our common "red spider." Now it is contrary to all scientific usage to name and describe a species from its immature characters; but the older authors not only described these six-legged mites as perfect animals, but referred the different forms to different genera. Therefore, as it is important that such common and annoying pests should have a "local habitation and a name," and as they are so far only known in the six-legged state, I shall provisionally, and for the sake of convenience, name them. Should any future arachnologist learn the true life history of either, he may, of course, recognize or reject these names as he sees fit.

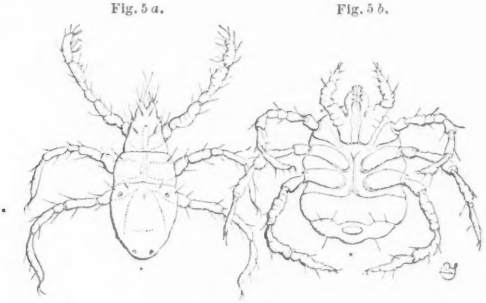
*The American Harvest mite** (*Leptus Americanus? n. sp.* Fig. 5 *a*). — This species is barely visible with the naked eye, moves readily and is found more frequently upon children than upon adults. It lives mostly on the scalp and under the arm-pits, but is sometimes found on the other parts of the body. It does not bury itself in the flesh, but simply insinuates the anterior part of its body just under the skin, thereby causing intense irritation, followed by a little red pimple. As with our common ticks, the

*Color brick-red, slender, ovate, the narrow, anterior end bifid, and furnished with stiff, converging setæ. Six-legged; legs long, the front pair blunt and slightly thickened at tip where they are incurved and thickly armed with stiff hairs; the others rather longer, and terminating in a stiff, curved, furcate claw. Average length ·008 inch.

irritation lasts only while the animal is securing itself, and its presence would afterwards scarcely be noticed but for the pimple which results.

*The Irritating Harvest mite** (*Leptus irritans, n. sp.* Fig. 5 *b*). This is the more troublesome and, perhaps, better known of the two, causing intense irritation and swelling on all parts of the body, but more especially on the legs and around the ankles. Woe betide the person who, after bathing in the Mississippi anywhere in this latitude, is lured to some green dressing-spot of weeds or grass! He may, for the time, consider himself fortunate in getting rid of mud and dirt, but he will afterwards find to his sorrow that he got hold of something far more tenacious, in these microscopic harvest mites. If he has obtained a good supply of

Fig. 5 *a*. Fig. 5 *b*.



Harvest Mites.

them, he will, in a few hours, begin to suffer from severe itching, and for the next two or three days he will be likely to scratch until his limbs are sore.

With the strong mandibles, and the elbowed maxillæ, which act like arms, this mite is able to bury itself completely in the flesh, thereby causing a red swelling with a pale pustulous centre containing watery matter. If, in scratching, the person affected is fortunate enough to remove the mite before it enters, the part soon heals. But otherwise the irritation lasts for two, three or four days, the pustulous centre reappearing as often as it is broken.

*Color brick, or blood-red; of tick-like form, being nearly as broad in front as behind; six-legged, the legs terminating in two stiff hairs; a strong pair of elbowed maxillæ, recalling a fourth pair of legs, and similarly terminating in two hairs; mandibles more or less distinctly tridentate at end inside. Length .01 inch.

The animal itself, on account of its minute size, is seldom seen; and the uninitiated, when first troubled with it, are often alarmed at the symptoms and at a loss to account for them. Fortunately, these little plagues never attach to persons in such immense numbers as do sometimes young or so-called "seed" ticks; but I have known cases where, with irritation and consequent scratching, the flesh had the appearance of being covered with ulcers; and in some localities, where these pests most abound, sulphur is often sprinkled, during "jigger" season, in foot-gear as a protection.

Sulphur-ointment is the best remedy against the effects of either of these mites, though when that cannot be obtained, saleratus water, and salt water will partially allay the irritation.

The normal food of either must, apparently, consist of the juices of plants, and the love of blood proves ruinous to those individuals who get a chance to indulge it. For unlike the true chigoe the female of which deposits eggs in the wound she makes, these harvest bugs have no object of the kind, and, when not killed at the hands of those they torment, they soon die — victims to their sanguinary appetite.

## ON THE GENETIC RELATIONS OF THE CETACEANS AND THE METHODS INVOLVED IN DISCOVERY.

BY THEODORE GILL, M.D., PH.D.

In a "Synopsis of the Primary Subdivisions of the Cetaceans," published in 1871,* I ventured some remarks on the apparent genetic relations of the Cetaceans, and observed that "between the Carnivores and the Cetaceans of the present age, the gap does indeed appear to be very great, but it is bridged over, to a very considerable extent, by the Zeuglodonts of the Tertiary epoch, . . . . and from the Zeuglodont stem have probably descended, in different directions, the toothed and whalebone whales; while the former, in some features, such as the general form of the skull, the teeth, etc., appear to deviate less from ordinary mammals; the latter, in other respects, but especially in the development of

*Proceedings and Communications Essex Inst., vol. vi, pp. 121–126.

the olfactory organ and of the nasal bones, depart less than they from the typical forms. It would therefore seem probable that the *Denticete* (Toothed whales) have become differentiated, as now recognized, little or not at all in advance of the *Mysticete* (Whalebone whales), or in other words that the latter are not offshoots from the former, but both from one original stock."

Dr. Brandt of St. Petersburg, to whom we are indebted for so many valuable memoirs in various departments of zoology, in a recent memoir on the classification of the Balænoidea* (or Mysticete), has misunderstood the tenor of these remarks, and supposing that I meant that the Balænoids (or Mysticete) and Delphinoids (or Denticete) were differentiated and developed from the Zeuglodonts in the Tertiary epoch, has expressed his dissent therefrom.

Such an interpretation illustrates the difficulty of expression so that there shall be no ambiguity. In view of my real sentiments, the interpretation in question struck me with astonishment on the first perusal, and at the same time appealed to my sense of the ludicrous. In season and perhaps out of season, in arguments with friends, and in public discourses, I have insisted upon the inadequacy of the palæontological record, and the absolute necessity, in view of our knowledge of the radical differences between the various types of animals, of extending the phylum of the various existing stocks into a most remote but necessarily indefinite past. I have even incurred the censure of geologists for insisting that the mammals, for example, must have been developed in a far earlier epoch than we have palæontological evidence of, and that even the palæozoic might not be too recent for their birth. The absurdity of the idea, that the specialized Denticetes and Mysticetes of the Tertiary epoch could have originated in that epoch and from tertiary Zeuglodonts, is such that it never occurred to me that it could be entertained by any scientific evolutionist, much less attributed to me. The remark that the gap between the Feræ and Cete is bridged over by the Zeuglodonts of the Tertiary epoch, and that from the Zeuglodont stem have descended the recent whales, certainly does not legitimately convey that idea, although, after consideration of the passage, I must confess that one unacquainted with any of my other writings might not be entirely in-

* BRANDT (Johann Friedrich). Ueber eine neue Classification der Bartenwale (Balaenoidea) mit Berücksichtigung der untergegangenen Gattungen derselben. . . . <Bulletin de l'Académie Impériale des Sciences de St.-Pétersbourg. t. 17, pp. 113–124, 1872; *also* < Mélanges Biologiques tirés du Bulletin. . . . . t. 8, pp. 317–333.

excusable for wresting such an interpretation therefrom, especially if my reference to their systematic places of the extinct typical Cetaceans was overlooked.

*Methods involved in discovery.* — In dealing with genetic problems, there are facts and inferences from facts to be considered.

As facts, the Zeuglodonts are less aberrant in structure and more related to the ordinary quadrupeds than are the existing Cetaceans, and they are not living, and their remains have only been found (or at least identified) in the Tertiary epoch.

As other facts, the Cetaceans of the present epoch share with the Zeuglodonts the special features which differentiate them as Cetaceans from other mammals, and superadd other specialized characteristics.

As facts, then, the Zeuglodonts (only yet known from tertiary beds) bridge over the gap between the Carnivores (or normal quadrupeds) and the existing Cetaceans, that is, they are more like the former than are the latter.

As inferences from these facts, it seems most probable that the known Zeuglodonts represent a stock relatively near the original stem or line of descent, and comparatively little differentiated (in at least the jaws, teeth, olfactory apparatus, members, etc.) from the generalized cetacean progenitors of the Denticetes and Mysticetes. Whether the restricted characters which might be applied to all the known Zeuglodonts could be extended to those atavan forms is questionable, but that the latter had the jaws, nasal apertures and teeth attributed to the suborder in my article is, I think, a perfectly legitimate inference from the facts and, therefore, it may with confidence be said that the Denticetes and the Mysticetes have originated from the generalized Zeuglodont stem (not Zeuglodonts) thus understood.

But when they originated is entirely another question, and for the solution of which we have no data. They — or one, or the other of them — may have become differentiated in the Cretaceous, or the Jurassic, or a still earlier age. I should probably in the main agree with Dr. Brandt, however remote he might place the date of origin* and at least would have no direct evidence to

*Especially as Dr. Brandt concedes that the Sirenians may have originated little before the Miocene (perhaps before the Eocene), with the Halitheriids as witnesses of the high degree of specialization as Sirenians which the Miocene forms had already attained. Generum Sireniorum, ab initio verisimiliter e formis inferioribus, species vel genera quædam Sireniorum, non alia animalia heterogenea (*Pachyaermata*), sensim

sustain an opinion one way or the other. It seems very safe, however, in view of the relations of the extinct faunas of that epoch to those of our own, to assume that it could not have been as late as the Cretaceous epoch.

*On so-called intermediate forms.* — Dr. Brandt, in connection with the subject in question, has taught us how the genealogical record should and should not be sought. "The hypothesis of the derivation from earlier, older forms," says he, "can only be proved with certainty directly from palæontology, and in no wise from so-called intermediate forms, which may have also originated independently, neither can it be, by means of analogy, indirectly deduced from isolated facts in the history of development."*

Here again, I am happy to find that on the whole I have not been entertaining very different views from the eminent master, and I accept the dictum (which I have often urged myself) that the genealogical line can only be proved (in its details) by reference to the actual forms, and that many so-called "intermediate forms" are themselves derivatives from the same common progenitors (at different removes) as the more specialized types.

But if it is really meant that the so-called intermediate forms do in no wise indicate the line and mode of descent of the more specialized types, I must for the first time differ, and differ decidedly, from my eminent critic. Do the Prosimians afford *no* hint as to how the Simians have originated? *None*, the Hipparions, the Anchitheriids, and the Palæotheriids for the Horses? *None*, the Oreodonts and the Anoplotheres, for the Ruminants? *None*, the Marsupials and Monotremes for the mammals? *None*, the Dinosaurians for the Birds? *None*, the Dipnoans for the Batrachians? *None*, the Marsipobranchiates and the Leptocardians for the Fishes? But why enumerate more of the hosts that crowd upon the memory for almost equal recognition? If such intermediate forms really give no clews or hints as to how more specialized and aberrant forms may have originated and developed, then indeed are facts in biology almost as barren and inconsequential

sensimque procreandi potentiam possidentibus, exorturum. origines itaque ante periodum miocænam (imo forsan adeo eocænam) transponendæ esse videntur. — Brandt, Symb. Siren., 1868, p. 371.

*Die Annahme der Abstammung von frühern, ältern Formen kann nur direct auf paläontologischem Wege mit Bestimmtheit nachgewiesen, keineswegs aus sogenannten Mittelformen, die auch selbst-ständige sein können, oder aus vereinzelten, der Entwickelungsgeschichte entlehnten Thatsachen auf dem Wege der Analogie indirekt abgeleitet werden. — Brandt, *op. cit.*, 332.

for the evolutionist as for the believer in patterns and special creations.

But I cannot believe that Dr. Brandt really means what he says: my familiarity with his previous works and train of thought forbids such a belief and I cannot doubt till I shall be authoritatively undeceived, that his words simply involve a too energetic expression of dissent from those (if there be such) who would believe that all so-called intermediate forms are exactly those in the line of descent from the more primitive to the more specialized ones. If this only is meant, I still find myself in agreement with Dr. Brandt, and admit that so-called intermediate forms do not necessarily prove the line of descent, but (if rightly so called) they do furnish all ranges of indication from a vague hint to absolute proof, according as they be more or less generalized, and more or less allied to those extinct forms in the regular line of descent, and by which can alone be demonstrated with certainty, according to Dr. Brandt, the lineage of any form. But how will Dr. Brandt avail himself of palæontology and identify and recognize, when found, those ancestral types? How approach it otherwise than by the same methods by which the "generalized" and "intermediate" characters are recognized? The great difficulty, indeed, consists in the identification of the forms in the direct line of descent; and the exact identification is practically impossible, but it may be sooner or later sufficiently approximated to give us tolerably satisfactory ideas as to the origin and successive differentiation of various types. And that end will be attained by the recognition of forms as successively intermediate as to structure and time of development, and thus it will be exactly by intermediate forms (and not the less so because revealed by palæontology) that the lineage will be proved!

*Taxonomic values of characters.* — Dr. Brandt further contends that the teeth, the olfactory organs, and the nasal bones have no determinative value.* And yet he gives the suppression of the teeth and the coördinate development of whalebone as the sole distinctive characters of the whalebone whales. Therefore, it is evident that he thinks that the teeth do furnish distinctive characters. He recalls the familiar facts that in early youth all Ceta-

*"Auf die Zähne kann kein entscheidendes Gewicht gelegt werden .. Dem Geruchsorgan, oder den Nasenbeinen vermag ich gleichfalls keinen Werth bei der Verleitung der Abstammung beizulegen."— BRANDT, *op. cit.*, 332.

ceans have teeth, while on the other hand, not only the whalebone whales, but also many Delphinoids, in old age, are wholly toothless, while others have only one or two teeth. And still he uses the want of teeth in the whalebone whales as a distinctive character. And thus I find myself still on the same platform with Dr. Brandt as to practice although he appears to differ theoretically.

The coördination of the want of teeth with other characters in the whalebone whales is invariable for the known forms, and may therefore be used as a diagnostic character. The want or presence of teeth *per se* is a character of little importance and of extremely varying significance. In the Rhytinids, for example, the want of teeth is only of family value; in the walruses, the hypertrophy of the canines and concomitant atrophy or suppression of the incisors are also only of family value; in the Artiodactyle Ungulates the want of (upper) incisors indicates less than subordinal distinction for one group (Ruminants) and in another case (Phacochœrids) scarcely specific distinction! But when the teeth *are* developed, their structure and relations do afford hints, and most suggestive ones, and the significance of similarity is more than in ratio to the continuing agreement of teeth of increasingly complicated structure.

As to the jaws and the teeth, as well as other parts, they are, it seems to me, as matter of fact, more similar in the Zeuglodonts to those of ordinary mammals than are those of the Denticetes or the Mysticetes, and they are at the same time coördinated with other characters less aberrant; in other words, they are in all essential respects more similar to the ordinary mammals than are the existing Cetaceans and, therefore, to use the favorite expression of Dr. Brandt, *ubi plurima nitent*, they are, inferentially, more nearly allied to and less divergent from the ancestral stem. If, however, it is denied* that they are more similar, I will only reply that I prefer to rely upon the evidence of my senses, and

* Nicht blos die Schädel der *Bartenwale*, sondern auch die der *Delphiniden* erscheinen nach meiner Ansicht im Vergleich mit den Schädeln der Landsäugethiere auf eigenthümliche Weise ziemlich *gleich anomal* und bilden zwei für den Aufenthalt im hohen Meere geeignete und dazu durch Naturgesetz bestimmte, selbstständige Schädeltypen, denen sich als dritter *gleichwerthiger*, zu den *Phocaceen* hinneigender Schädeltypus, der der *Zeuglodonten* anschliesst.—BRANDT, *op. cit.*, 331.

This passage is *apropos* of my remarks respecting the intermediate character of the Zeuglodonts quoted in the introduction to this article. The only comment I shall venture shall be in the form of a question. If the Zeuglodonts incline towards the Phocacea in their skull, why are they not to that extent (less their own deviation from the direct lineage) intermediate between the recent whales and the Phocacea?

even if the facts do not appeal to the senses of another in like manner, still do I prefer to trust to my own.

*Inferences respecting genetic relations.*—The question having been raised as to the comparative degrees of differentiation of the cetaceous types, it may be well to pursue it further.

Zeuglodonts. As already observed, the Zeuglodonts, in the form and structure of the jaws, the character of the teeth (molars double-rooted in part), the presence of the typical (Educabilian) number of teeth in the intermaxillary bones, the more or less anterior position of the nostrils, the contour of the skull and general relations of its constituent elements, and in fact almost all the known parts of their organization, differ much less from the ordinary mammals than do any of the existing Cetaceans. They are therefore the most generalized or the least specialized Cetaceans known; these are simple facts which appeal to the senses. As inferences, the forms so distinguished represent, better than any other Cetaceans, the primitive ones from which they, as well as the latter, have descended. None of the known Zeuglodonts can, indeed, be the progenitors of the modern Cetaceans, since types closely related to the latter are associated with them in tertiary strata, and the known Zeuglodonts may have become much differentiated (possibly even more than the modern Cetaceans), in some minor points, from the primitive forms, but that they are, as a whole and in all essential features, more like (and therefore more allied to) those ancestral types can scarcely be doubted, *me judice*. Therefore those Zeuglodonts may appropriately be regarded as the nearest known representatives of the Protocetacean types, as quasi-intermediate forms between the quadruped mammals and the more specialized Cetaceans, and in a genealogical system must be represented as the nearest of kin to the prototypes of the order.

But even the few forms of Zeuglodonts known differ in degrees of differentiation from the normal mammals, and must be so represented, the *Basilosauriids* representing a more generalized and the *Cynorcids* a more specialized type.

Mysticetes. It seems more probable that the agreement of the Mysticetes and Denticetes in the attenuated intermaxillaries, the anterior nostrils, pectoral members, etc., should be the result of inheritance than of independent assumption, and therefore that they have developed from forms thus differentiated from the primitive Zeuglodont stem.

As to the forms most generalized, serious doubts may be entertained. The Denticetes have almost universally been considered as entitled to that rank, and if the form of the jaws and the teeth are alone considered, such would seem to be undoubtedly the correct view. But in other respects (such *e. g.* as the relations of the bones around the calvarium, the frontals, the posterior portion of the maxillaries, the development of the lachrymal, the less atrophy of the pelvis, the rudimentary hind limbs) the Mysticetes appear to me to be the most generalized, and, although the evidence may be vague and inconclusive, I may be permitted, till contrary evidence supervenes, to represent such apparent probability in a genealogical system. Of the two families (*Balænopteridæ* and *Balænidæ*) known, the Balænids appear to have superadded to the Mysticete type the most specialized feature and most generalized characters, such, for example, as the orbital prolongations of the frontal bones, the reduced coronoid processes of the lower jaw, etc.

Denticetes. Respecting the families of Denticetes the evidence is also vague, but hints are furnished by various structural characters. These may be illusive, but in default of evidence to the contrary, and until superseded, may be followed. It may be that other parts would furnish conflicting testimony, that there may be an unusual persistence of primitive characters in some regions, while in some others the structure has been much modified, and it is even not impossible that there may have been a reversion to ancestral characteristics in certain parts, but until such deviations are proved, it seems most in accord with sound philosophy to take provisionally, and in default of other, the *prima facie* evidence offered. With these remarks, the succession of the various families of Denticetes may be sought.

In the first place, two forms present themselves, each of which presents claims for the nearest representation of the ancestral line —the Iniids and the Ziphiids. The Iniids, and their near relatives the Platanistids, offer in their comparatively long neck and free vertebræ testimony in favor of such title, while the Ziphiids, in the development and continued independence of the lachrymal bones, produce theirs. And it seems very much more credible that these characters should have been inherited without fault than that they should have been the result of reversion after once having been lost, especially as there appear to be no offsets to such

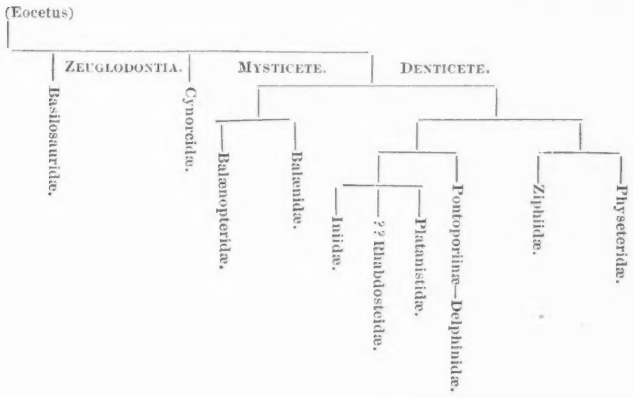
characters, and the rest of the organization is not in disaccord with those evidences of generalization.

On the whole, it appears to me that the long-necked Cetaceans represented by the living Iniids and Platanistids and in greater number by various forms in the Tertiary epoch are best entitled to the first rank. Whether of those, the Iniids or the Platanistids are the first is equally uncertain, but as the latter are certainly in some respects the most specialized, to the Iniids may be conceded the rank provisionally.

Probably, as more differentiated offshoots from the same secondary stem as the Iniids and the Platanistids, may be considered the Delphinids, of which the Pontoporiinæ doubtless represent the most generalized form.

Recommencing with the other secondary stem, apparently the Ziphiids represent the oldest rank, and the Physeterids are the results of an offshoot from the same lineage.

I have thus endeavored to present my views, and I trust that the language I have employed may prevent me from being misunderstood to mean that any one of the known specialized forms is derived from another of the known specialized forms. I have simply essayed to indicate what now appear to me to be the proximate relations of the several forms, and respectively the more generalized of the approximated groups. The following table may more vividly convey my views; in each case, the left branch indicates the supposed most generalized and the quasi-oldest form:



I shall only add that I have no intense convictions of the correctness of this representation, and regard it as simply provisional and subject to the modifications which the accumulating testimony now being so rapidly wrested from the living and the dead may necessitate. I do believe, however, that it is not in opposition to the data which have up to the present time been collected and tabulated. The advantages of such tables, in bringing into synoptical form and impressing upon the mind the various degrees of relationship and subordination of the respective subdivisions of a group, appear to me to be equally obvious (although not equally pregnant with meaning), whether we are evolutionists or patternists.

*Remarks on Dr. Brandt's classification.*—A few words on nomenclature and on the subfamilies of Mysticetes may be advisable.

Dr. Brandt* implies censures, by an exclamation mark (!), on the name Mysticete, and the inference conveyed thereby, and by his language, would be that I was responsible for the introduction of the name. As to the name itself, I perfectly agree with Dr. Brandt that it is objectionable and I hesitated sometime before adopting it. It was, however, the first introduced (by Gray, in 864†) and for that reason and that alone, I have employed it. It seems strange that Dr. Brandt should have been ignorant of this previous introduction, as he has referred to Gray's works in his memoir. I adopt very many names that are objectionable to me, recognizing as I do the inexorable demands of priority,‡ nor do I consider it necessary to protest against every inapt or ungrammatical name thus adopted, or found in the works of others, such, for example, as *Kyphobalæna* and others adopted by Dr. Brandt. §

As to the subfamilies, Dr. Brandt has suppressed those admitted by myself and others among the Balænopterids adding, however,

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*Eine dritte, neueste, von Th. Gill vorgeschlagene, Classification der Bartenwale, die er *Mysticete (!)* nennt, etc.· Brandt, Mel. biol., viii. 317.

†Gray, Proc. Zool. Soc. 1864, p. 198. It is true that Brisson had before called the same group *Cetacea edentula*, and Wagner, *Cetacea edentata*, but neither of those names fulfilled the requisites of nomenclature.

‡Lest I may be here, too, misunderstood, I add that I simply recognize the rule of priority because of the advantage afforded as a basis for uniformity of nomenclature, and am not influenced in the slightest degree by any considerations of "honor" or "justice" to nomenclators.

§ *Cetotherium*, *Cetotheriopsis*, etc., are employed in the same memoir by Dr. Brandt.

two for extinct types, *Cetotheriinæ* and *Cetotheriopsinæ*. But while suppressing the subfamilies, he has retained the characters, the want of which induced me to frame one of them, in the diagnosis of the family itself. In other words, the subfamily Agapheliæ was named for forms of Balænopterids distinguished by the absence of pectoral folds and of a dorsal fin, yet Dr. Brandt, while suppressing it as unworthy of subfamily distinction, considers the development of such folds and of a dorsal fin as family characters.* The development or not of the folds and fin is certainly not of family value and should therefore be eliminated from the definition of the family, as it misleads both as to the prevalence of the characters and their value, and at the same time diverts the identifier from the path. Whether the characters are of subfamily value is another question, and one which need not be discussed here.

In conclusion, it appears that I share the opinions of Dr. Brandt on most of the questions discussed, and I am happy to find that I can enroll myself under the banner of so able a leader; and I decidedly protest against being held responsible for views which I am as willing to oppose as he. As to the other points in which we appear to differ, I am fain to believe that it is due to the use of language more comprehensive than was meant by Dr. Brandt, and with the disposition to exercise that allowance for ambiguity which I would wish to have practised in respect to myself, prefer to surmise his real views from the general tenor of his works and thought, than to accept his exact phraseology.

## REVIEWS AND BOOK NOTICES.

Archæological Collections in America.—The recent report† by Prof. J. Wyman on the specimens received by the Peabody Museum in Cambridge is a most instructive document, as it not only gives a list of the additions made to the Museum during the year but also contains much interesting information relating to

*Pectus et abdomen sulcis longitudinalibus exarata. Pinna dorsalis perfecta vel tuberculo repræsentata. — Brandt, Mel. Biol. viii. 326; see also p. 321.

† Fifth Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology. Presented to the President and Fellows of Harvard College, May 15, 1872. 8vo pamphlet, pp. 35. Boston, 1872.

the specimens received; combining many notes by the eminent curator of the museum, suggested while comparing and arranging the immense collection which has been gathered by the careful management of Prof. Wyman and the other trustees of the museum. When the building fund shall have accumulated to an amount sufficient to enable the trustees to erect a proper building in which to exhibit the treasures in their charge, there will be opened a museum of archæology and ethnology that will have but few rivals in size and excellence. The foresight exhibited by the trustees, in obtaining the large and valuable foreign collections as they have been offered for sale, has secured the means of direct comparison of the relics of the prehistoric races of the old world with those of the new. Prof. Wyman remarks, when speaking of the Clement collection, the balance of which was received during the past year:

"The Museum may be considered fortunate in its acquisitions pertaining to European Archæology. With that of Clement from the Swiss lakes, that given by the Museum of Comparative Zoology from the same source, and to be mentioned further on, that of de Mortillet from France, Switzerland and the Italian lakes, with the admirable and very complete collections by Wilmot J. Rose from Denmark, Schleswig and Holstein, and with that of Claus from the same countries, we now possess good means for the study of European Archæology, and for the comparison of the implements and objects belonging to the early age of man in Europe, with the analogous ones of the new world. In view of the fact that there exists a large demand for archæological objects in the principal museums of Europe, that the Danish government prohibits the exportation of such, that the ancient dwelling places on the Swiss and Italian lakes, as also the caves and rock shelters of France, have been largely explored, and many of them exhausted, it is hardly probable that opportunities for obtaining collections, such as those above referred to, will be again offered to us."

In regard to the collections from America the trustees have been equally active and have received many valuable additions, especially from the labors of Rev. E. O. Dunning in East Tennessee, and those of the curator himself in Florida, beside the direct donation of many specimens from various parts of the country, including over eight hundred specimens from New Jersey presented by Dr. C. C. Abbott, who has also done so much for the archæological department of the Academy at Salem.

In his remarks on the Clement collection, Prof. Wyman makes many allusions to the similarity of the specimens with those from

this country, and when mentioning the great numbers of antlers of deer and the implements made from them, states:

"It is worthy of notice that in this collection a large part of all the antlers in which the base remains, were not such as came from animals killed in the chase, but such as had been dropped at the period when they were annually shed, as appears from the peculiar surface of the bone on the line of separation due to absorption. The horns of the deer seem to have been as great a mine of material to the lake dwellers for the manufacture of useful articles, as flint to the ancient inhabitants of Denmark, stone to the North American Indians, or bone to the Esquimaux and the natives of the northwest coast of America."

To show that similar results are attained generally by similar means, we quote a few lines on the drilled stones from the Clement collection:

"The method of drilling is well illustrated in a variety of instances, some showing the action of a solid, and others of a hollow rotary drill. Some of the last were not finished, but broken perhaps in the act of making, and the place from which the core was detached is quite obvious. A few of the cores are preserved. We thus have, as Mr. Rau has pointed out, processes of drilling parallel to those used by the Indians of this continent."

We may add that in the Academy collection there is a specimen received from New York, which shows the core about a quarter of an inch high standing up from the bottom of a hole that had evidently been drilled for two or three inches by a hollow drill before the specimen had from some cause been broken.

The letter of Mr. Dunning relating to his explorations in Tennessee and the account given by Prof. Wyman of the specimens collected are of great interest to the students of American archæology, and correspond in several respects with the account given by Dr. Jones in a former number of this journal. Among the most interesting relics found in the Tennessee mounds were a number of carved shells which Prof. Wyman describes in his report, of which we hope to be able to give figures in a future number.

As an instance of the acute examination which the curator gives to the specimens that come under his charge, we quote the following remarks on pottery ornamentation:

"A large proportion of all the vessels as well as fragments are in one way or another marked with the impressions of twisted cords. Similar markings have been observed on pottery from very distant parts of the United States, and have been observed on the

earthen vessels of the prehistoric period of the old world. We have specimens from Maine, Massachusetts, Missouri, Illinois, Ohio, Tennessee and Florida. It is an interesting fact that, while every trace of the cords and woven textures made by the mound-builders has perished, we have impressions or casts of the first left with sufficient distinctness on their earthen vessels to determine the style of twisting and the number of strands, and of the second to ascertain, in some cases, at least, the manner in which the cords were interwoven. By means described further on, the exact structure of the impressing surface has been reproduced. The explanation usually given of these markings is that the vessels have been moulded in a net, which was used to support the soft clay while the process of manufacture was going on. That vessels, especially larger ones, were moulded in baskets, and these destroyed in the burning, there is an abundance of evidence, as set forth in Mr. Rau's interesting paper on the pottery making of the North American Indians in the Smithsonian Report for 1866. This is a point about which there is scarcely any liability to error. But there is a great difference between moulding a vase in a firm and steady structure like a basket, and a yielding, flexible one, like a net. None of the specimens we have thus far received show that a net, if by net is understood a structure formed of meshes made by knotted cord, was used in moulding a vessel, for no impression of a knot is to be found anywhere. It would have been if any existed, as we have shown experimentally. The impressions are, in all cases, either of a *woven* texture or else of cords neither knotted nor woven but probably wound about some body, and in this form used as a stamp. By making casts of the surface of the cord-marked vessels with gutta percha, we have reproduced the original details of the impressing surface, which show very clearly the above differences. The textures are of two kinds, one with and the other without open meshes. The first are formed by a series of parallel cords or warps, intersected by a second series of parallel cords crossing the first at right angles, but including one of these in every twist of its strands. The laborious process was therefore required of passing the two strands of which the second cord is made above and below the first cord, and then twisting them before passing to the next. The texture with closed meshes is handsomely woven, and in one instance of threads not exceeding a thirtieth of an inch in diameter. Unfortunately, none of the vessels bearing markings of a woven texture are entire, so that it is impossible to ascertain whether the impressions are distributed in a uniform manner over the whole surface. It seems incredible that even an Indian would be so prodigal of time and labor as to make the necessary quantity of well twisted cord or thread, and weave it into shape for the mere purpose of serving as a mould, which must be destroyed in the making of a single copy. It must be remembered that the vessels are all made with bodies more or

less bulging or spherical, and that in consequence, if formed in a mould, this must either be made in sections capable of being separated, or else it must be destroyed either by cutting or, as is more commonly supposed, by burning, before the copy could be removed. There appear to be no traces of sections, and the impressions show no signs of a mould adapted to removal. Possibly the vessels thus ornamented were intended only for special purposes, as for religious ceremonies or the use of chiefs, and were not made in very large numbers, and so an unusual amount of labor might be accounted for. The second form of cord marked pottery is more common, and is very easily understood. The cords were arranged for the most part parallel to each other, and not connected either by weaving or knotting. We have reproduced such impressions by winding a cord around a stick, and pressing this against the surface of the clay, stamping only a limited surface at one time. In order to cover the whole surface in this way it would be a matter of necessity that adjoining impressions would interfere with each other more or less, which they actually do on the surface of the vessel, one set partially obliterating another. Such impressions must therefore be regarded as finishing touches after the vessel was formed rather than as casts of a mould in which they were supposed to be made. This view is sustained by the fact that they often extend on to the handles, which are never added until the body of the vase is completed, and also by the fact that some of the impressions are but faintly made, as if the clay had already become somewhat hardened before the cords were applied. In one case the impressions were such as would be made by a ball of loosely wound cord, rolled over the surface. We are unable to say whether such markings had more than an ornamental signification, but it is worthy of notice that they were so largely used in widely different parts of the country. We saw similar markings on a vase in the Museum at Berlin, marked as to its origin *unbekannt*, unknown, in which the cord marks were arranged in a few horizontal circles and vertical lines, obviously taking the place of the ornamental lines usually traced with a pointed instrument. Sir John Lubbock mentions the existence of vases from ancient mounds in Scotland, ornamented with impressions from twisted thongs, and further states that in the stone age 'the most elegant ornaments of their vases are impressions of the finger nail, or of a cord wound round the soft clay.' Smith. Rep., 1862, p. 320. In view of these facts the question arises whether the impressions of the finer woven fabrics may not have been also merely ornamental markings added after the vase was completed, and not impressions of a mould in which they were formed."

In recording the collection made by Mr. Dunning from the burial caves in Tennessee, first noticed by Dr. Jones in the Naturalist, Prof. Wyman says:

"A second cave is situated near the mouth of the Big Pidgeon River, not far from Newport, in Cocke County. As described by Mr. Dunning, 'it is about eighty feet above the water, and reached only by a steep rocky path called Devil's Gap. The tomb was found about two feet below the floor of the cave, covered with an artificial layer of clay about six inches in thickness, by which the joinings of the stone were completely closed. It was five feet long, two high and three and a half broad, and built of unhewn stones, fragments of the outcropping limestone ridge near by. The body was placed in a crouching position. Charcoal and ashes were present, indicating that fire had been kindled near the tomb. The only relics found buried with the skeleton were about five pounds of disks made from some large marine shell from an inch to an inch and a half in diameter, and perforated in the middle.' The skeleton found in this stone tomb, as appears from the imperfect ossification of the bones, was that of an individual not quite adult, having a height of nearly six feet, but with bones of rather slender make. The tibiæ are somewhat flattened, and the fore arms are much lengthened, in proportion to the upper arm, the radius being 0·81 and the ulna 0·87 of the length of the humerus. The cranium was not quite perfect, but sufficiently so to determine its principal proportions. The most marked feature, and this is very striking, is the extreme artificial flattening of the occiput, and the consequent increase of the diameter of the head from side to side, so that the breadth somewhat exceeded the length, a degree of distortion not often met with even in the extreme cases among the Peruvians. In many of the North American Indian tribes a comparatively slight amount of distortion is often met with, but among a few it was carried to an extreme condition, as in the Natchez, as recorded by Adair and Bartram, and more recently by Morton; among the Choctaws and Waxsaws, according to Lawson, and among the Catawbas, according to Morton."

It is interesting to know that we have the flattened form of head in the ancient race of Tennessee as well as the natural form, for in a skull which Dr. Jones obtained from East Tennessee, of which we have a photograph in the Academy collection, the high forehead is a marked feature, and it seems now to be a fact beyond dispute that both forms of crania, as expressed by the terms of high and low foreheads, are common throughout the whole mound region of the United States, indicating a great similarity with the ancient races in Central and South America.

The third section of this interesting report contains an account of Prof. Wyman's own explorations in Florida. The care with which the professor renewed his examination of the shell heaps he had formerly so faithfully explored is most valuable in giving unques-

tionable data to other explorers. We have not space now to quote from this part of the report except so far as relates to the age of the mound at Silver Spring, a large shell heap of from two to twenty feet in height and said to cover an area of about twenty acres. This heap is made up almost entirely of the small fresh water shells of the genera Ampullaria and Paludina, and, as Prof. Wyman remarks, it seems incredible to conceive that such vast numbers of small shells could have been brought together by man from the waters about, and the immense size of the mound must be regarded as the work of many years and probably of centuries.

"There is to be seen at Silver Spring a grove of live oaks, a few survivors of a race of giants once common in the forests near the river, and to which my attention was called by my friend G. A. Peabody, Esq. Six of these at five feet from the ground measured as follows: one thirteen feet, three fifteen, one nineteen, and one between twenty-six and twenty-seven feet in circumference. This last has been partially destroyed by fire, an act of vandalism committed for the purpose of collecting the moss hanging from its branches. The circumference was estimated from one-half of the trunk, all that now remains, but agrees closely with measurements made several years before by Mr. Peabody, when the trunk was still whole. These trees are not on the highest part of the mound, but on the slope farthest from the water. Excavations made beneath the largest of them showed that the tree was of more recent origin than the mound itself. If at the beginning of the second century of the life of the live oak there are twelve rings at least to the inch, then the above mentioned tree, having a semidiameter of fifty inches, would have an age of not less than six hundred years, and was near the beginning of the second century of its existence at the landing of Columbus. On the same basis of calculation, the least age of the mounds near Blue Spring, and at Old Town, would be about four hundred years. Though these estimates are to be regarded only as approximations to the truth, they, without doubt, carry back the origin of the mounds beyond the reach of history or tradition, and certainly one or two centuries before the discovery of America. Although they cannot be more recent than the trees growing upon them, they may have been, and probably were, finished long before the life of the trees above mentioned began."

Revision of the American or Tyrant Flycatchers.*— This revision of the *Myiarchi* is based upon all the accessible material

*Studies of the Tyrannidæ.— Part I. Revision of the species of Myiarchus. By Elliott Coues. Proc. Acad. Nat. Sci. Phila. 1872, pp. 56-81. July, 1872.

in this country, numbering over two hundred specimens, and comprising the entire suites of the Smithsonian Institution, Museum of Comparative Zoology, and Mr. Lawrence's collection, and an examination of the types in the collections of the Boston Society of Natural History and the Academy of Natural Sciences of Philadelphia, together with numerous specimens from other sources. In this paper Dr. Coues has adopted the "synthetic" method of investigation instead of the "analytic" which, up to the present time, has been so generally followed, especially by American ornithologists. It is hence a paper of unusual interest as fairly initiating a "new departure" in American ornithology. Dr. Coues here takes the "arbitrary" but apparently justifiable basis of predicating "'species'† upon specimens presenting any definite, constant, tangible characters whatsoever, that do not, so far as it appears, grade into the characters of other species;" of predicating "'varieties' upon specimens presenting indefinite and inconstant yet tangible characters that are seen to grade into the characters of other specimens;" of predicating "'synonymes' upon specimens presenting indefinite, inconstant, and intangible characters, due to individual peculiarities, or to age, sex, season or locality; as well as upon specimens presenting no special characters at all." His investigation of the genus has led him to the belief "that there are only four forms (*sic*) of Myiarchus that do not intergrade, and that are differentiated from a common original stock to such degree, or in such manner, that we cannot account for their respective peculiarities according to highly probable laws of geographical variation depending upon differences in food, climate, etc." He finds that the specimens examined by him "represent nine species, two of which present each three tangible varieties." These results are somewhat different from those reached by other investigators of the group, and in allusion thereto he observes: "though in the following pages I may appear to have 'unnecessarily,' if not unwarrantably, reduced the number of species, yet I am persuaded that no unprejudiced ornithologist could have reached different conclusions upon study of the same material. It may be well to remember that two hundred specimens of Myiarchus have never before been examined by one person at a *coup d'œil;* and I think that with two thousand

† Compare Bull. Mus. Comp. Zool., III, p. 127, July, 1872.

specimens instead of two hundred, I should not be able to establish as many species as are here allowed."

The species and varieties recognized are the following: 1. *Myiarchus validus*, known only from Jamaica. 2. *M. crinitus*, with three localized varieties, viz., *crinitus*, which ranges throughout the eastern portion of the United States and retires to Central America to winter; *irritans* (including *Mexicanus* and *Yucatanensis* Lawr.), inhabiting Central and South America to Paraguay and distinguished with difficulty from var. *crinitus; Cooperi* (*Tyrannula Mexicanus* Kaup) confined chiefly to southern and southwestern Mexico. 3. *M. cinerascens* (*Mexicanus* Baird), "one of the better marked species of this difficult group" inhabiting southwestern United States and Mexico. 4. *M. tyrannulus* (*ferox*, *Swainsonii*, *Panamensis*, etc. auct.) a homogeneous type, ranging over Central America and southwest to southern Brazil. 5. *M. phæocephalus* of Ecuador, suspected to be a local race of the preceding. 6. *M. Lawrencei* of Mexico and Central America. 7. *M. nigriceps*, of Central and northern South America; though a tangible species, regarded as "simply a geographical representative of *M. Lawrencei*." 8. *M. stolidus*, a flexible species, with three insular varieties or local races: viz., *stolidus*, Jamaica, St. Domingo and Hayti; *Phœbe*, Cuba and Bahamas; *Antillarum*, Porto Rico and Tobago, the Porto Rican form being very strongly marked. 9. *M. tristis*, Jamaica. Not only have all these "varieties" ranked hitherto as species, but others reduced in this paper to synonymes have currently held similar rank.

Preliminary to a revision of the species, the leading features of the genus are clearly sketched, as distinguishing it among allied genera. It proves to be a not sharply defined group, "the genus so called" resting "upon no structural characters, while its synonymes are among the vagaries of ornithology." A few species usually relegated to other genera are shown properly to belong here, and the genus as thus defined is susceptible of a tolerably definite diagnosis. Before proceeding to an analysis of the species our author discusses other general matters relating to the subject, especially individual and geographical variation, and announces several propositions to which he invites serious consideration. The importance of some of these will warrant their repetition here as being an exposition of important facts and principles at present engaging the attention of ornithologists, and capable of wide application.

"The normal inherent variability, in size, of the whole bird and its members, is at least twelve per cent. of the mean. (This is independent of all extraneous circumstances.)"

"Size varies in direct ratio with the latitude of the breeding season."

"Size of peripheral parts, as compared with total size, varies in inverse ratio with the latitude of the breeding-place. (*Cf.* Allen, Bull. Mus. Comp. Zool. II, p. 229)."

Intensity of coloration varies in direct ratio with the temperature and humidity of the breeding-place. Moisture, however, intensifies color more than heat; aridity tones down color more than cold. Birds from hot dry places, therefore, are paler, *cæteris paribus*, than birds from wet places of the same or even lower temperature. (*Cf.* Allen, *op. cit.*, p. 239)."

"Variation, unconnected with age, sex or season, is in inverse ratio with the migration or changeable geographical distribution of individuals."

Other propositions are announced relating to variations dependent upon age and sex in the group especially under consideration. They all appear to have been strictly followed, and the conclusions thus reached seem to be in the main thoroughly tenable. The propositions relating to geographical variation, though as yet far from being generally accepted, we are convinced are well founded, as the more thoroughly they are tested the more fully are they confirmed.— J. A. A.

Monograph of the Spheniscidæ.*—In this important memoir of forty-two pages we have one of the most valuable contributions to the literature of the *Spheniscidæ* that has yet appeared. It opens with a critical historical synopsis of all preceding papers treating of the group, from Linnæus down to the present year, in which are briefly yet lucidly traced the principal changes of synonymy and the gradual accumulation of our present knowledge of the family. From the two species known in 1766 to Linnæus, the number had increased in 1781 to eight valid species, four of which were then made known for the first time by Forster in his valuable history of the group. The next valid new species was described by Brandt in 1837, "the first for half a century." Later the number of valid species was increased to twelve, the number recognized by Schlegel in 1867, and by our present author. Respecting Schlegel's judicious revision of the group Dr. Coues observes; "As far as the determination of the species is concerned,

* Material for a Monograph of the *Spheniscidæ*. By Dr. Elliott Coues, U. S. A. Proc. Acad. Nat. Sci. Phil., 1872, pp. 170-212, with 8 woodcuts. (Sept., 1872.)

our own studies bear out Dr. Schlegel's in every single instance; indeed, it seems to us impossible to reach any other conclusion, when any considerable and sufficient amount of material is examined. The present article of ours is so completely an endorsement of Dr. Schlegel's, that the only points of difference are one or two unimportant synonymical determinations among the crested species which, after all, will probably remain matters of opinion."

The materials on which Dr. Coues' memoir is based are the collections of the Philadelphia Academy of Natural Sciences, (now for the first time elaborated), and of the Smithsonian Institution. Both are rich in representatives of this group, with which have been also collated the specimens in the Museum of the Boston Society of Natural History. Part II is devoted to a discussion of "certain points of cranial structure bearing upon the determination of the genera." Alluding to the diversity of opinions among authors in respect to the number of genera — some placing all in one genus and others making a genus of each species — Dr. Coues states that "to fix the question of genera with reasonable certitude" was one of the objects of his present investigation. An examination of the skulls at his command (but representing only a part of the species) showed "*three* positively different patterns." Each pattern, while marked by peculiarities of its own, possesses characters shared also by one of the others, and it is on the combination of these features that the genera are established. Whilst our author thinks it "probable that no more than three genera will be finally determinable, namely, Aptenodytes, Eudyptes and Spheniscus," he provisionally admits a fourth, Pygocelis. "These genera are exactly those of Prof. Hyatt," and "correspond very nearly with the sections Dr. Schlegel has indicated." In this connection the chief osteological peculiarities of *Aptenodytes* "*Pennantii*" are described, with more especial reference, however, to the membral segments. "The tarso-metatarsus," Dr. C. remarks, is the most remarkable bone of the skeleton in several respects, and the one more particularly diagnostic of the family; penguins afford probably the only instance, among recent birds, of width crosswise being decidedly greater than thickness antero-posteriorly, and more than half the length; and the only case of persistence throughout life of fenestræ marking the composition of the bones of three originally

distinct metatarsals." These membral and cranial features are illustrated by several figures drawn by Prof. Morse.

Part III treats briefly of the geographical distribution of the species. The penguins are not only confined to the southern hemisphere, but range northward only to latitudes 10° south on the Pacific coast of South America and to 8° south on the Atlantic coast of the same continent; on the African coast only to 25° south and occur only much further to the southward on the coast of Australia. The Falkland Islands appear to be the geographical centre of the family, where no less than half the species occur. They range southward, however, as far towards the pole as voyagers have yet penetrated. The species have usually a wide range, several of them being circumpolar; of none does the exact range of periodical movements or migrations appear to be known. In general they assemble in immense numbers at their breeding stations where they commonly remain for but a short portion of the year.

Part IV gives a list of the species, with their synonymy, and Latin diagnoses. The specimens examined are enumerated, and generally each is described more or less in detail, with special reference to an elucidation of the various stages of plumage each species presents. As we have already indicated, only twelve species are recognized, as follows:—*Aptenodytes Patagonica*, *A. longirostris*, *Pygocelis taeniata*, *P. adeliæ*, *P. antarctica*, *P. antipodes*, *Eudyptes catarractes*, *E. chrysocome*, *E. chrysolopha*, *E. diademata*, *Spheniscus minor*, *S. demersus*, *S. demersus* var. *Magellanicus*.

This elaborate memoir constitutes a valuable supplement to Prof. Hyatt's recent catalogues of the *Spheniscidæ*,* and must form for many years a standard work of reference for the group. Besides elucidating the complicated generic and specific synonymy of the family, it is a valuable contribution to our knowledge of the osteology of the penguins, and to their geographical distribution and changes of plumage during the period of adolescence.—J. A. A.

DUBOIS' CONSPECTUS.†—Lists of European birds seem destined to occur at frequent irregular intervals, and perhaps we cannot have too many of them, at any rate so long as they continue to agitate the subject by their notable mutual disagreements, and thus serve

*See Amer. Nat. Vol. VI. pp. 472, 545.

†Conspectus systematicus et geographicus Avium Europæarum; auctore ALPH. DUBOIS, etc., etc., Bruxelles, 1870. (8vo. pp. 35.)

to keep us alive to the requirements of the case. While we personally have not the particular information required for nicety of criticism in such an instance as the present, we may, nevertheless, indicate the general features of the paper. The author has limited his field to "Europe," politically speaking, as is customary indeed, but as is not, in our judgment, either necessary or desirable. As naturalists, we should consider the distribution of our objects of study with reference rather to natural faunal areas, at least when the species of more than a single locality are to be collated. We trust that the compiler of the next "European" catalogue will take this into consideration. Prof. Dubois catalogues five hundred and seventy-five species in gross, under two hundred and fifty-three genera of fifty families, this enumeration being exclusive of numerous "varieties," but inclusive of the "stragglers" (*fortuito occurrentes*). There are, we find, about one hundred and sixty-five of the latter, leaving four hundred and ten species net. Comparing this with a rather recent list * of very excellent authority, the discrepancy is notably slight, Prof. Blasius giving four hundred and twenty regular inhabitants, one hundred and three casuals and fifty-five varieties. The totals of the two lists (five hundred and seventy-five and five hundred and seventy-eight) are surprisingly close, but it should be remembered that this apparent agreement is largely brought about by accidental counterbalancing of numerous individual discrepancies; and furthermore, if Dr. Dubois had, like Prof. Blasius, numbered the geographical and other varieties he admits, the result would have been very different. On the whole, we cannot consider that European ornithologists have as yet reached unanimity in the cases of more than two-thirds of the species that occur within their limits. Whether the present list is more or less reliable than some of its predecessors, we must leave to the judgment of those who are better informed than ourselves; but there is no doubt of its very general acceptability.

Much may be said in general terms, in favor of the classification of this brochure, although we cannot endorse it throughout. We protest, as other writers have, against the "fissirostral" association which places swallows alongside swifts and goatsuckers; we see no grounds for the uniting of American *Tireonidæ* with the old world *Muscicapidæ*, nor the propriety of putting the nine-

† Blasius; Newton's ed. of 1862.

primaried American *Sylvicolidæ* under *Sylviidæ*.* We have little faith in the desirableness of associating the cuckoos with the woodpeckers in a group *Zygodactyle*, greatly preferring Huxley's definition of the Coccygomorphs. In the matter of nomenclature we are not at one with the author, who goes back for his names to Ray, Gesner, Willoughby and Aldrovandi, to say nothing of the comparatively late Brisson and Moehring; but this is simply a matter of individual preference. Whatever "rules" may be made, they are only binding at our option — paraphrasing an old saying: *inter synonyma silent leges.** *

[We take this occasion to request ornithologists to favor the NATURALIST with a copy of any paper they may hereafter publish; intending to devote reasonable space to the respectful consideration, at the hands of our ornithological co-laborers, of such publications.— EDS.]

NEW ENGLAND ORNITHOLOGY.— Mr. Maynard contributes a very acceptable and creditable paper,† increasing our knowledge of the summer northern distribution and breeding habits of many species of which comparatively little was before known; and gives good descriptions of various nests and eggs. The information respecting most of the land birds observed is quite full and apparently perfectly reliable. The species given number one hundred and sixty-four, which is probably about five-sixths of the whole avi-fauna of the regions explored. As the author confines himself to his own personal observations and those of a few gentlemen who have worked in the same or contiguous localities, the paper is notably free from misstatements of fact, although some of the generalizations seem to us somewhat overdrawn if not altogether hasty. We are unable to agree with Mr. Maynard respecting certain flycatchers which he discusses at length. He evidently labors under a misapprehension (shared, we understand, by other New England ornithologists) regarding *Empidonax Acadicus*. This bird, which appears to be hardly known in New England, is per-

*Respecting this family we are informed by Dr. Coues that he considers it inadequately distinguished from *Turdidæ*, viewing the annectant forms of the two families; and that the current *Turdinæ*, *Saxicolinæ*, *Regulinæ*, *Miminæ*, *Pycnonotinæ*, *Sylviinæ* (*Sylvia*, *Erythacus*, *Accentor*, *Calamoherpe Phyllopneuste*) should form one family, which may require to be further enlarged to accommodate the *Troglodytidæ* and *Motacillidæ*.

† A Catalogue of the Birds of Coos Co., N. H. and Oxford Co., Me., with annotations relative to the breeding habits, migrations, etc. By C. J. Maynard. Proc. Bost. Soc. Nat. Hist. xiv, October 1, 1871.

fectly distinct from *Traillii* and *minimus*, between which Mr. Maynard misconceives it to stand. As an example of the faulty reasoning with which we must charge the author, we may cite the case he presents of *E. minimus*. Finding *a certain amount* of variation in the proportions of the quills, he assumes that the wing-formula is *entirely* unreliable; which is not the case. If, for example, he had said of *E. minimus* "second, third and fourth quills subequal and longest, fifth little shorter, first and sixth subequal and shortest," he would have laid down a formula by which the species is always distinguishable from *Acadicus** (not from *Traillii*, however). Reverting to a matter of more consequence, we should note that in the localities visited by Mr. Maynard "the Alleghanian and Canadian faunæ meet. . . Starting on the northeastern coast of Maine, near Mt. Desert, the dividing line of these faunæ proceeds in a southwesterly direction along the southern margin of the mountain range which stretches across the state to the White Mountains. Here it declines to the south, reaching even to Rye Beach. Then once more proceeds northwest along the western borders of the mountain range into Vermont. . . So abruptly is the line defined in many places by the range of mountains, that some birds which occur in abundance on one side are found only as stragglers, or not at all on the other."

For the numerous typographical errors which deface the paper we understand that the author cannot be held responsible, since he had no opportunity of revising the proofs. The paper itself is such a forcible commentary upon the inexcusably faulty practice, by far too common, and quite needlessly so, of printing scientific matter without author's revise, that we refrain from the sermon which nevertheless we are strongly inclined to preach on this occasion. — E. C.

Annals of Bee Culture.† — This annual contains several essays of great interest and value to bee keepers; they are all good, and some of sterling value, and apparently above the average of articles appearing in the ordinary bee journals. Its ap-

*The formula of *Acadicus* is: second and third quills subequal and longest, fourth little if any shorter, first and fifth subequal and much shorter, sixth much shorter still.

† Annals of Bee Culture for 1872. A Bee Keeper's Year Book. D. L. Adair, editor. With communications from the best American Apiarians and Naturalists. Louisville, Ky., 1872. 8vo, pp. 64.

pearance encourages us in the hope that bee keeping will be conducted on a more scientific basis than ever before in this country.

Underground Treasures; How and Where to Find Them.* The design of this little book is to make every farmer and landowner his own mining engineer, and when his knowledge is exhausted to induce him to go to some professional mining engineer for advice. Perhaps the recent diamond swindle demonstrates the need of just such a guide as this. The plan seems well carried out, the descriptions of minerals, ores and gems being terse and clear, and the hints as to how to find them are practical. After describing the eighty minerals which out of two hundred and forty-four found within the United States are of practical use, the author gives chapters on "Prospecting for Diamonds, Gold, Silver, Copper, Lead and Iron," "Mineral Springs," "Artificial Jewelry—How Made and How Detected," "Discovery of Gold in California," and a concluding one on the "Discovery of Silver in Nevada."

## BOTANY.

Past Vegetation of the Globe.—Nine years after the publication of Brongniart's "Tableau" Dr. Paterson discovered, in a bituminous shale near Edinburgh, *Pothocites Grantoni*, which has been generally accepted ever since as a monocotyledonous flowering plant. It can therefore no longer be asserted that in the Palæozoic period the higher Phanerogams were absent. Nor can it be even said that, amongst Phanerogams, Pothocites belongs to a very primitive type. The condensation of its inflorescence and the reduced structure of its flowers imply, on any hypothesis of evolution, the previous existence of flowering plants which had undergone less differentiation. Indeed, for anything that can be positively said to the contrary, there may have been during the Carboniferous epoch a phanerogamic covering to the earth hardly less complicated than there is now. Our knowledge of the vegetation of that time is confined to the forests of arborescent Cryptogams fringing the deltas of great rivers. Stems of coniferous trees were occasionally floated down from the higher ground; of the plants that grew with them we know nothing.

*Underground Treasures: How and Where to Find Them. A Key for the Ready Examination of all the Useful Minerals within the United States. By Prof. James Orton. Illustrated. Hartford, Conn. Worthington, Dustin & Co. 1872. 12mo, pp. 137.

Still less can it be said of the Mesozoic period that its fossil remains convey any adequate notion of the contemporary *facies* of the vegetation. The cones and driftwood that occur in rocks of marine formation of this age would have been little injured by immersion in water in which the flowers and foliage of less rigid plants would speedily have decomposed beyond recognition. Such guesses as we can make about the actual vegetation of Mesozoic land surfaces stand in the same relation to the reality as do those which a traveller would make in approaching a new country from the ocean, and in collecting the vegetable waifs and strays borne out to sea by currents, to the estimate which he afterwards forms when he botanizes at leisure on the land itself. It is, however, only fair to admit that if arborescent Dicotyledons existed to any large extent anterior to the chalk, it is hardly explicable that we have as yet no evidence from driftwood that this was the fact, except Mr. Sorby's notice of some non-gymnospermous wood from the Lias near Bristol,* which appears to have been overlooked. In the "dirt-bed" of the Upper Oolite we have a true land surface, but the ligneous plants of this were undoubtedly gymnospermous. It is far from improbable however that, at any rate, herbaceous Dicotyledons had made their appearance in the Mesozoic period. Monocotyledons, as already pointed out, are certainly known to date from a time still earlier, and in the herbaceous condition Dicotyledons are less different from Monocotyledons than when they become woody. Several facts seem to prove that existing trees are more modern than herbaceous plants belonging to the same groups. They have, for example, more confined ranges, and often represent on oceanic islands, apparently because the exaltation of their stature has had less to struggle against, orders which elsewhere comprise only herbaceous plants. Probably in every group the arborescent habit has been a subsequent development. — W. T. Thiselton Dyer in *The Academy*.

Seeds as Projectiles. — *Editors of Naturalist:* Allow me the favor to correct the phraseology I, by some unaccountable slip of the tongue, employed in referring to the Hamamelis seed. It is the contracting of the horny *endocarp* not the horny "albumen," which projects the seeds.—Thomas Meehan.

* Transactions Microscopical Society, vol. iii, p. 91.

How the Buffalo Grass Disappears.—Prof. Mudge in an interesting letter in the "Kansas Farmer" on northwestern Kansas, gives some interesting facts as to the gradual disappearance of the buffalo grass and the incoming of other grasses before the advent of civilized men. He says:

"The steadiness and regularity of this change is interesting. Seventeen years ago the buffalo grass covered the hills and prairies about Manhattan, but it has been gone many years. Six summers ago, when we first visited the forks of the Solomon, we found it everywhere except close to the river bank. Two years later, the blue stems had possession of half the bottom. Now the buffalo grass has entirely left the latter ground, and is fast vanishing from the high prairie. In November, 1866, we visited Smith and Phillips counties, then unsettled, and found buffalo grass in full possession, but this summer it had disappeared to the extent of one-half in the bottoms, and the tall grasses had become intermingled with it. On the high lands the change had already begun, but to a limited extent. On the Prairie Dog and at the upper portions of the Middle Fork, we found the change just commencing. In crossing from Cedarville to Bull City in Osborne county, we noticed that the buffalo grass had left the divide to the extent of one-third, and the coarser grasses above named had taken its place.

We thus record a few of these changes, that others may notice the regularity and rapidity of the disappearance of the buffalo grass."

Hepaticæ Cubenses Wrightianæ. — Under tickets with this heading Mr. Charles Wright has distributed a few sets (varying from two hundred to one hundred and fifty species) of *Hepaticæ* collected by him in Cuba several years ago. They have in the meantime been studied by Gottsche of Altona, who is the principal authority in Hepatic mosses, and are named by him. The authentic names are given upon the tickets. The sets are to be disposed of, at ten dollars the hundred specimens, upon application to Mr. Wright, at the Herbarium of Harvard University.

A Grand Herbarium. — The herbarium of Columbia college, New York, is to have added to it the immense collection of Dr. Meissner, the distinguished Professor of the University of Basle. This herbarium contains 63,000 species, and is purchased for the college through the liberality of J. J. Crooke, Esq., a wealthy amateur scientist. The present herbarium of the college is the invaluable one of Dr. John Torrey, and is especially rich in

typical specimens. With the proposed addition it is said that it will be the largest herbarium in the country.

## ZOOLOGY.

CEMIOSTOMA AGAIN.—In my note *ante*, p. 489, I have stated that in the "Transactions of the London Entomological Society" Ser. 2, vol. 5, pp. 21 and 27, and in Ser. 3, vol. 2, p. 101, certainly two, and if my memory serves me aright three, species of Cemiostoma have been described from India. These references were evidently made from memory. It seems from Mr. Mann's note *ante*, p. 606, that but two species are mentioned on the pages referred to and those two are from England, not from India. Nevertheless, I am still convinced that my memory is not utterly at fault, and that species of Cemiostoma have been discovered in India, and when the opportunity again offers I will look them up. Many months had elapsed after I saw the "Trans. Lond. Ent. Soc." before my note on p. 489 was written, and probably I have confounded in my mind the above references with some other. Eastern naturalists surrounded by fine collections, libraries and every facility for study can scarcely appreciate the difficulties with which their less favored western brethren have to contend; and Mr. Mann no doubt learned whilst in Brazil that want of the means of reference to what others have done is a very different thing from "negligence."

Cemiostoma, Phyllocnistis, many species of Lithocolletis and a few other genera of Tineina have a spot *in the apical part* of the wing which I have therefore called "the apical spot." In Phyllocnistis and in Lithocolletis this spot is always *at* the apex: but in Cemiostoma it is always at the inner angle. So characteristic of each of these genera is the position of the spot in it, that when the name of the genus is given and its spot is mentioned, the student who is familiar with the genus knows at once where the spot is located; just as Mr. Mann knew at once from my description the location of the spot in *C. albella*, although he had never seen the species and although I called it, for brevity, and not through negligence, "the apical spot" instead of "the spot located at the inner angle." But if the phrase "apical spot" might have been misleading had it stood alone, it could not have been so in the description of *C. albella*, because it is connected with the state-

ment that "*behind it at the base of the cilia* is a fuscous streak" showing that the "apical spot" was not at the extreme apex.

*C. susinella*, *C. coffeella* and *C. albella* are evidently very nearly allied, if they are not in fact different names for the same species. All of Mr. Mann's figures in his last plate will answer *for some specimen of albella*, especially the figure of the cocoon. The mode of pupation is the same, and I have been able to detect no differences in the larvæ. *C. albella* and *C. susinella* mine the leaves of poplars, and *albella* also mines those of willows; whether *susinella* does is not ascertained. The identity of the food plant, and the close similarity of the insects, raise a strong presumption that they are the same species. But *albella* has the tuft on the vertex as distinct as it is in *coffeella*, whilst Mr. Stainton says that *C. scitella* was (when he wrote) the only European species which has such a tuft, and if so, then *susinella* must be distinct from both *albella* and *coffeella*. But Mr. Stainton's note upon *susinella* is very brief and he does not pretend to give an accurate or detailed description of it. Besides, it has not yet been found in England, and Mr. Stainton's specimens must have come from Europe, and therefore may be a little worn, and the tuft is very easily obliterated. Mr. Stainton's brief note, then, scarcely affords sufficient data for a comparison with other species. He says that *susinella* has two fuscous streaks pointing upwards in the cilia, represented, as I infer, by the last two streaks in Mr. Mann's figure; and *susinella* and *coffeella* therefore do not differ in this respect; but in *albella* the first of these last two streaks, that immediately behind the "apical spot," is only fuscous at the costa and the remainder of it is pale golden. The outer fuscous stalk is represented in *albella* by the fuscous spot at the apex, and with the cilia expanded as in flight this spot would become a streak. In *coffeella* the spot is partly surrounded (on the sides towards the base and towards the costa) with pale golden. Mr. Stainton does not mention this golden margin, but he says that the first (golden) costal streak is continued to the anal angle (where the spot is), and if so, it must partly surround the spot. Mr. Mann represents the two golden costal streaks as not attaining the spot, and not confluent with its golden margin. My description of *albella* was drawn up from four specimens in which I failed to detect the presence of the golden margin around the spot thus differing from *coffeella* and in which the golden costal streaks did not attain the

spot thus agreeing with *coffeella* and differing from *susinella*. Since the description of *albella* was written I have obtained many specimens and find a greater range of variation than I then supposed to exist. In some specimens the golden margin around the spot is only visible in some lights, in others it is distinct and wide, so as to be confluent with both golden costal streaks, and I have a specimen in which this is the case as to one wing, whilst on the other both streaks are entirely distinct from the golden margin of the spot. If the same range of variation exists in *coffeella* and *susinella* I do not see how they can be regarded as distinct species, nor wherein they differ from *albella* except that in *albella* the ciliary streak is golden, except on the costa where it is fuscous, whilst in the other two species it is said to be entirely fuscous. Possibly, however, they may differ as to the spot itself. For Mr. Stainton says that in *susinella* the spot is black with a violet ocellus, whilst in *albella*, although the color varies with every change of the light, I would not call the central part of the spot an ocellus at all, nor its color violet; but would rather consider the spot as brilliant silvery, or silvery-gray, metallic, margined distinctly with black before and behind, and but faintly or not at all above and beneath. I doubt, however, the specific difference of the specimens, and if they are distinct the difference can probably only be determined by a comparison of a large series of specimens of each.— V. T. C.

## GEOLOGY.

Proboscidians of the American Eocene. Correction.— Having for the first time obtained a view of the premaxillary and maxillary bones of the *Eobasileus cornutus*, I find that the tusk which I have called an incisor is a canine.— E. D. Cope.

Return of the Yale College Expedition.—Professor Marsh and party returned on the 7th of December from the Rocky Mountains, where they have spent the last two months in geological researches. They bring back a large number of vertebrate fossils from the Cretaceous and Tertiary formations of the West, including many new and interesting mammals, birds and reptiles. Among the treasures secured during the present trip was a nearly entire skeleton of *Hesperornis regalis* Marsh, the gigantic diving bird of the Cretaceous; a second species of *Ichthyornis* (*I. celer*

Marsh), and numerous remains of Pterodactyls. The new fossils will soon be described by Professor Marsh.

NOTICE OF A NEW AND REMARKABLE FOSSIL BIRD.—One of the most interesting of recent discoveries in palæontology is the skeleton of a fossil bird, found, during the past summer in the upper Cretaceous shale of Kansas, by Prof. B. F. Mudge, who has kindly sent the specimen to me for examination. The remains indicate an aquatic bird, about as large as a pigeon, and differing widely from all known birds, in having *biconcave vertebræ*. The cervical, dorsal and caudal vertebræ, preserved, all show this character, the ends of the centra resembling those in *Plesiosaurus*. The rest of the skeleton presents no marked deviation from the ordinary avian type. The wings were large in proportion to the posterior extremities. The humerus is $58{\cdot}6^{\text{mm.}}$ in length, with the radial crest strongly developed. The femur is small, and has the proximal end compressed transversely. The tibia is slender, and $44{\cdot}5^{\text{mm.}}$ long. Its distal end is incurved, as in swimming birds, but has no supratendinal bridge. This species may be called *Ichthyornis dispar*. A more complete description will appear in an early number of this Journal.—O. C. MARSH, *American Journal of Science and Arts.*

KNOWLEDGE OF PETROLEUM IN PENNSYLVANIA IN 1771.—On page 638 of the October number of the NATURALIST is a notice of the fact that petroleum was known to exist in Pennsylvania in the last century, and the date given was about 1789. I have in my library "Kalm's Travels in North America" in which is a map "published according to act of Parliament, March 7, 1771," upon which I find marked "petroleum" on the Alleghany River about eight miles above the mouth of French Creek. The locality is marked with a little cross (+) on the east bank of the river, which would put it very nearly opposite to the mouth of Oil Creek as now known. I also find on the same map, in what is now Ohio, in the vicinity of the present location of New Philadelphia in Tuscarawas County, "Coals and whetstones:" and on the Hocking River near the southern portion of the state is found the word "coals."

Kalm makes no mention of either coals or petroleum in these localities; in fact, he did not himself travel so far to the west, but the fact of these names being on a map published in 1771 shows

that they must have been known for a considerable time prior to that date. — C. E. Bessey.

On an Eocene genus allied to the Lemurs. — Professor Cope recently read a paper before the American Philosophical Society on an extinct mammal from Wyoming which he called *Anaptomorphus æmulus*. The number of teeth in the lower jaw is precisely the same as in man and the higher apes, but their structure is nearer that of certain Lemurs at present existing in Madagascar and East Africa. This resemblance is closer than has yet been discovered to exist in any fossil genus, but is somewhat diminished by the separation by suture of the two halves of the lower jaw. The animal was as large as a squirrel.

Fossil Monkeys. — Dr. Forsyth Major has just published in Italy an account of certain fossil Simian remains which have lately been for the first time discovered in Italy, and which he refers to a species closely allied to the Barbary ape, *Macacus inuus*, still found at Gibraltar. To this account the writer appends a history of all fossil Quadrumana at present known. Of these, seven species belong to Pliocene and Quaternary, ten to Miocene, and three to Eocene strata. No fossil Lemuridæ have yet been discovered; the fossils as yet found in S. America belonging to the Platyrrhini, still peculiar to the Neotropical region. All the rest belong to the Catarrhini, and some to the anthropomorphous genera; these have all been found in the old world, but while some occurred in India, others inhabited France, Germany, Greece and England. — A. W. B.

On Some of Professor Cope's Recent Investigations. — In the Naturalist for November last (p. 669), Prof. E. D. Cope has a paper on the "Coal Beds of Wyoming," in which he claims to have made the discovery that these strata are of Cretaceous age. This, however, was already known to every one familiar with the geology of that region. The existence of Cretaceous coal in various parts of the Green River basin had previously been established by Mr. Meek, Messrs. King and Emmons, and myself, although Professor Cope makes no reference to our researches. Any one wishing to consult the recent literature on this subject will find it cited in the "American Journal of Science" for December 1872, page 489.

In the December NATURALIST (page 773), there is another paper by Prof. Cope on the "Proboscidians of the American Eocene." The discoveries here claimed rest on an equally unsatisfactory basis. The species mentioned had apparently all or nearly all been previously described by Dr. Leidy and myself, the type species, *Tinoceras anceps* Marsh, dating back to June, 1871. Some of the characters given by Prof. Cope, *e.g.*, the large upper incisors and absence of canines, do not, indeed, apply to the species I have described; but I feel quite sure that Prof. Cope's haste has unfortunately led him to mistake canines for incisors. On several other points, especially the position of the horns and structure of the skull, I believe Prof. Cope to be equally wrong. The animals described evidently belong to the order which I have called *Dinocerea* (Amer. Journ. Sci., Oct. 1872, p. 344). Their true characters and affinities, I propose soon to discuss fully elsewhere. — O. C. MARSH.

DISCOVERY OF EXTINCT MAMMALS IN THE VICTORIA CAVES, SETTLE, YORKSHIRE.—This famous bone-cave has hitherto produced only remains of different ages from the Neolithic period to the present. Recent excavations have yielded, however, at the depth of about twenty feet, bones of the elephant, rhinoceros, hyena, a crushed canine of a much larger carnivore, etc. The elephant's teeth found belong to a young individual, and the number of gnawed bones and other indications, that the cave had been a den of some larger carnivores, render it probable that the elephant was dragged into it by them. — A. W. B.

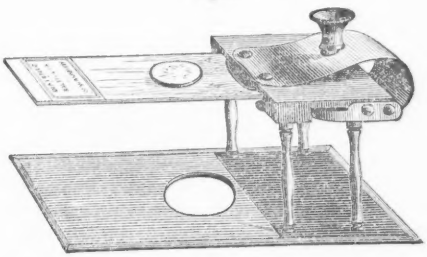
## MICROSCOPY.

MICROSCOPY AT THE VIENNA EXPOSITION.—The Exposition of the Industry of all nations to be held in Vienna this year, will afford microscopists a rare opportunity to exhibit to the world the results of their ingenuity in contrivance, or of their skill in construction, of microscopical apparatus and appliances. General T. B. Van Buren is Commissioner General for the United States, and President F. A. P. Barnard is chairman of the Advisory Committee on Group XIV, in which are included optical instruments. Persons desirous of contributing to the exhibition of American art on this occasion are requested to communicate immediately with any of the following persons who are the microscopical

members of the committee; Profs. R. H. Ward, M.D. of Troy, New York, H. L. Smith of Hobart College, Messrs R. B. Tolles of Boston, Mass., W. S. Sullivant of Columbus, Ohio, J. B. Rich of New York City, William Wales of Fort Lee, New Jersey, Charles A. Spencer of Canastota, New York, Joseph Zentmayer of Philadelphia, Pennsylvania, and J. Grunow of New York City.

A New Accessory Stage. — Messrs. James W. Queen & Co., of Chestnut Street, Philadelphia, and Broadway, New York, have contrived a stage which can be used with any microscope and which will commend itself to many microscopists as a very useful accessory. It consists of a brass stage-plate, perforated in the centre for the transmission of light and bearing, at one end, four pillars which support, at the height of about an inch, a second plate. To the under side of this second plate the object-slide is attached by

Fig. 6.



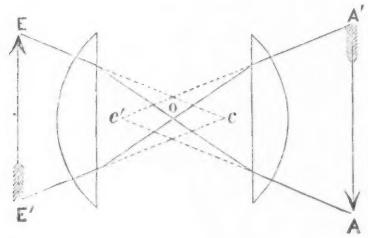
means of slight springs which allow it to be easily misplaced. It is evident that this contrivance admits of any degree of obliquity of illumination without regard to the construction of the stand on which it is used; and the slight awkwardness of adapting an achromatic condenser to this apparatus is nearly negatived by the fact that most microscopists prefer to obtain extremely oblique illumination either by a prism, or directly (unmodified) from the source of light, for both of which this arrangement is especially available. The comparative safety of the thin glass cover over the object will also be appreciated by the many persons who have seen a rare or costly object, such as the Type Plate, or Nobert's Lines, ruined by an incautious touch of a high power objective.

Magnifying Power of Objectives. *To the Editors of the* American Naturalist. Dear Sirs: — With great interest and pleasure I have followed the preliminary movements to establish a

uniform nomenclature of the value of achromatic objectives for the microscope, to which the foremost microscopists of our country and abroad have advanced their contributions.

The problem is a complicated one, and the following will by no

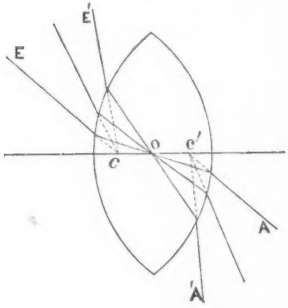
Fig. 7.




means diminish the practical difficulties, but will only add one more which has not been brought into consideration.

Undue importance is given to the optical centre of a lens, or combination of lenses, by the different writers upon the subject, while the great importance of the conjugate centres of a lens has been entirely neglected. The conjugate foci of a lens or combination of lenses, are in no way dependent on its optical centre, but entirely on the conjugate centres. The single plano-convex lens makes an exception: for in this the optical centre and the conjugate centres fall together, where the optical axis meets the curved surface.

Fig. 8.




If we take, for instance, a double convex lens of equal radii, Fig. 7, its optical centre is O, and consequently the rays A′ and A, striking the lens at such angles as to pass through the optical centre, will emerge at E′ and E, parallel with the first directions. If now the rays A′ and A are prolonged towards the optical axis

of the lens, they meet at a point, C′, the centre of admission. If the rays E′ and E are prolonged, they will meet at C, the centre of emission. Therefore the conjugate foci do not meet at the optical centre O, but are to be measured from C′ to the object, and from C to the image; and the sum of the conjugate foci is *not* equal to the distance between object and image, but in this case the distance between C and C′ must be deducted.

In combinations of lenses it is precisely the same. It is almost impossible to analyze such a complicated system as a modern microscopical objective, and to fix the position of the optical centre or the conjugate centres, although all combinations possess these remarkable centres. But let us take a simple combination of two plano-convex lenses placed symmetrically, in which it is not difficult to determine all that we need. In such a combination, Fig. 8, the rays A and A′ pass through the optical centre O, and emerge to E and E′, parallel with their original directions. Now if we prolong A and A′, they will meet at C′, the centre of admission; and E and E′ prolonged will meet at C, the centre of emission. To find for this combination the relation of conjugate foci, or the relation between the size of object and image, we have to compare the triangle ECE′, with the triangle A′C′A. In this case the sum of the conjugate foci is equal to the distance of object and image, plus the distance from C′ to C. In combinations this will generally be the case. — Joseph Zentmayer, *Philadelphia, Sept. 25th*, 1872.

Amphipleura Pellucida by Moonlight. — Many microscopists have had the curiosity to use the beautiful white light of the full moon as a source of microscopical illumination, but probably few have tried it upon the more difficult objects. Prof. T. D. Biscoe, led on by the clear sharp view given by it of easier objects, tested it upon the last diatom of the Test Plate, using a Hartnack objective No. 10, and resolved the "test" at first trial.

The Study of Lichens.—The explanation of the peculiar double nature of the lichens has lately become the subject of much discussion. It has been long recognized that in the tissue of lichens, are to be found two quite distinct classes of elements. By one class the lichens are allied with the fungi, by the other with the algæ. The great body of a lichen is made up of a structure exactly identical with certain fungi, while scattered through the

substance are green granules or cells called gonidia; these bear a strong resemblance to certain kinds of algæ.

The same double nature of the lichens is evinced in their fructification, even more strikingly than in the simple vegetative system. The complete identity of fruit (apothecia and spermagona) produced by hyphen threads of lichens with fruit of the division Ascomycetæ of the fungi has been well known, and has even led to the classification of this division of fungi with the lichens (by Schleiden in 1842). But what astonishment was created when, in 1867, Famentzin and Baranetzky showed that the gonidia also, in favorable circumstances, produced fruit identical with the zoospores of algæ.

The question presses home more and more, whether the lichen is a single individual whose development follows these two divergent paths, or whether two distinct individuals out of different natural classes have combined together to live a united life.

On the former supposition, the complete agreement of the gonidia of lichens with certain algæ, and the fact that gonidia freed from the lichen threads in which they lie embedded possess the power of independent life and development (in which state they cannot be distinguished from algæ): these two considerations have led to the almost inevitable conclusion that numerous genera of algæ (as supposed) are undeveloped or, it may be, abnormal states of lichens. Famentzin and Baranetzky have lately adopted this theory. On the other hand, De Bary (1866) has pointed out the possibility that in the case of the "jelly-lichens" (Gallertflechten) the gonidia may be real algæ which have assumed the form of lichens because parasitic fungi (of the family Ascomycetæ) have united themselves with them.

Since 1867 Schwendener has extended this theory over the whole class of lichens. According to him lichens consist of algæ spun over, and swallowed up, as it were, in the meshes of the mycelia of certain fungi. There seemed one thing only needed to establish this theory, namely, to succeed in raising lichens by sowing the spores of fungi on gonidian-like algæ. This experiment has been successfully carried through in the case of a given species of the genus *Collema*, by Dr. Beess in 1871.

Although this would seem to close the case, yet the new view is not accepted by the most experienced lichenologists. They hold the view of the single nature of the lichen, saying that the

resemblance of gonidia to algæ does not prove identity, that they have microscopically demonstrated the genetic connection of the gonidia with the hyphen threads of that lichen, and that Tulasne has raised lichens from lichen spores, without the presence of any algæ; hence the Berlin Academy has announced the following Prize-question: "The proving of Schwendener's view of the double nature of the lichen," by means of original investigations. And they recommend the study of the following points.

1st. An exact study of the numerous one-celled forms of algæ which so closely resemble the gonidia of lichens. These are now classed in the genera *Pleurococcus*, *Cystococcus*, *Glococystis*, etc.

2nd. Continuous investigation on the gonidia contained in the thallus of lichens, especially with regard to their development after being freed from the lichen thallus for the purpose of ascertaining with more certainty the different types of algæ that appear. The question whether among the great number of green gonidia, inhabiting lichens, there may not be more numerous types than has been supposed, taken in connection with the investigations suggested above on the free living forms of algæ ought be kept clearly in mind. The case of the occurrence of different forms of gonidia in one and the same lichen deserves special attention.

3d. The carrying on of repeated "culture-from-spore" experiments with lichens from different families with and without the presence of the algæ that are supposed to be the nourishing plants. This should be especially done with lichens containing chlorophyl-green gonidia.

The work may be presented in German, Latin, French, English or Italian. Important points of investigation must be illustrated by drawings, and the presentation of preparations (microscopic) is advisable.

The time for sending in the papers is fixed at the first of March, 1875. Real names are to be sent in sealed envelopes. The prize is one hundred ducats.—T. D. B.

Misnaming Objectives.—[Mr. Wenham has made public the following brief reply to Mr. Stodder's communication on this subject in the August number of the Naturalist. This controversy, having already called sufficient attention to the points at issue, would be fruitless if still further prolonged.] I should

not have taken time to notice the long comment on my short letter, appearing on page 234 of the "Monthly Microscopical Journal" for May, 1872, but for the remark that my letter was written with "evident loss of temper!" Quite the reverse; it was penned in a spirit of "chaff," and Mr. Bicknell, in his brief note in reply, seems to have caught the vein; at which no one, perhaps, laughed more heartily than myself. On the other hand, it has drawn C. S. out of his shell, with horns erect, in his proper name or color. I have nothing further to say on the question, which leads to no scientific discovery, and is one to be settled between the makers of object-glasses and purchasers, who are now sufficiently warned. No particular reform can be anticipated by pages of controversy having for its very basis such full scope for personalities, of which this and the above may be taken as a sample. The tone is becoming silly and tiresome; and having contributed my share, I must drop the subject with the remark that no one would be more willing to induce the makers to adopt a nomenclature having a definite reference to actual magnifying power than myself, could I see the possibility of doing so. Numerals such as those adopted by the Continental makers would perhaps partly meet the difficulty; but I believe that no English optician would consent to name his glasses this way.—F. H. WENHAM.

NEW YORK UNCINULÆ.—Mr. Charles H. Peck has communicated to the Albany Institute a synopsis of the New York (State) Uncinulæ, described seven species as occurring in the state in addition to two described by Dr. E. C. Howe. Only three species are credited to Great Britain, whose mycology has been well investigated. Our species are systematized as follows.

Appendages to the conceptacles thirty or more.

Sporangia with eight spores,.....*U. circinata.*
Sporangia with six spores.....*U. parvula.*
Sporangia with four spores.....*U. adunca.*
Sporangia with two spores.....*U. macrospora.*

Appendages less than thirty.

Appendages white, flexuous toward the tips....*U. flexuosa.*
Appendages white, not flexuous.....*U. Clintonii.*
Appendages colored,.....*U. ampelopsidis.*

Dr. Howe's species are *U. Americana* (*U. spiralis* B. & C.) figured but not described by Berkeley, which is near *U. ampelopsidis* but has appendages few, longer and colored; and *U. luculenta* which is

much like *aduncа*, but has fewer and longer appendages and sometimes sporangia with five or six spores.

STAINING VEGETABLE TISSUES. — L. Erckmann explains, in the "Journal of the Franklin Institute," that the staining of plant sections with a weak solution of aniline red, and then washing out with water the color from all the non-nitrogenous parts, is not only useful for purposes of general study, but is especially applicable in the preparation of specimens for photographic use.

## NOTES.

A semiannual session of the National Academy of Science was held at Cambridge, November 21st, 22d, 23d, in the lecture room of the Museum of Comparative Zoology, where Professor Agassiz welcomed the members, and gave an account of the rise and present condition of the museum. Of the twenty-eight papers read there were presented thirteen relating to geology and zoology, with the following titles:—

The Organization of the Museum of Comp. Zoology in Cambridge, by L. AGASSIZ.
On three different Modes of Teething among Selachians, by L. AGASSIZ.
The Development of Actiniæ, by ALEX. AGASSIZ.
The glacial Phenomena of the southern Hemisphere compared with those of the North, by L. AGASSIZ.
Affinities of Echinoderms and Worms, by A. AGASSIZ.
Notice of Investigations making in California on the Reliability of the Barometer as a hypsometric Instrument, by J. D. WHITNEY.
Pedicellariæ of Echinoderms, by A. AGASSIZ.
Results of recent Dredgings on the coast of New England, by A. E. VERRILL.
Embryological Fragments concerning the Volutidæ, by L. AGASSIZ.
On the specific Identity of some Animals along the Atlantic and Pacific shores of America, by L. AGASSIZ.
The copulatory Organs of the Selachians compared with one another, and with those of other Vertebrates, by L. AGASSIZ.
On the changes Selachians undergo with age, by L. AGASSIZ.
Critical remarks about scientific views entertained upon theoretical grounds, by L. AGASSIZ.
Notice of the progress of the topographical work of the Geological Survey of California, by J. D. WHITNEY.

Professor Agassiz read a paper on "Three Different Modes of Teething among Selachians." He said that in former years he had paid considerable attention to the peculiarities of teeth among the Selachians, but the progress of zoology and palæontology made the present materials on hand quite insufficient. It was not known what changes took place with age. So he had determined upon

the voyage of the Hassler to make the collection of Selachians a principal object. He had been richly rewarded for his efforts. Since his return he had made careful examination of the collection, comprising sometimes two hundred specimens of one species. The result of this examination was that while in their adult condition the Selachians present characters which are very constant among specimens of the same age, there are such changes among them that even genera have been founded on the difference of age. Professor Agassiz then illustrated from abundant specimens and upon the blackboard the variations of dentition in Selachians of different ages from the embryo to the adult. In concluding he alluded to the relation which the facts of variation he had presented might falsely be supposed to sustain to the development theory. The conditions which occupied a certain place in the series to be derived one from another should be consecutive in time. This was not the case. It was the endless series of anachronisms which were being made by the supporters of the transmutation doctrine which had kept him aloof from all such interpretations of Nature. When it should appear that these different features fall in time as they may appear to fall in their connection by similarity, then there would be some ground for the inference of a gradual change. Geologists ought to be as careful in their generalization as were physicists. He thought that there was too much loose twaddle and argument and debating-club demonstration in our Natural History. He had been told recently by one who occupies a very high position in science that "unless you deduce one being from another you are not following a legitimate scientific course." It should first be proved geologically that there is such a genealogical connection. The facts show, indeed, something that should not be overlooked, viz.: that there is thought in nature, and until it is proved that thoughts are derived one from another, he would not admit that the similarity of two objects proves their derivation one from the other.

Mr. Alexander Agassiz made a communication on the "Development of the Actiniæ."

The second day's session opened with an account of the glacial phenomena of the southern hemisphere compared with those of the north, by Prof. Agassiz. Any one who had been familiar with the glacial phenomena as exhibited in the northern hemisphere, both in Europe and the United States, and who would have ac-

cepted, even with considerable limitation the general conclusions he had presented concerning the glacial period, might have foretold, said Prof. Agassiz, that the southern hemisphere would present the counterpart of all these phenomena. And yet he supposed that many of his friends thought he was over-sanguine when, in a letter to the Superintendent of the Coast Survey, he had told what he expected to find, in this matter, during the Hassler Expedition. The hesitation which was prevalent concerning these generalizations arose from the view which many entertained of the true cause of the phenomena. Many thought that the greater extension of glaciers in the Alps and most parts of Europe was to be ascribed to the former existence of large sheets of water in the north of Africa, from the evaporation of which great amounts of snowy deposition could be formed upon the Alps, and thus enlarge the glaciers. But he would ask those who entertained this view how a sheet of water in Africa could have made great sheets of ice upon the continent of North America? There had been a disposition more or less outspoken among geologists to view the phenomena of the greater extension of glaciers in a former period as the result of local glaciers. He believed he was the only one among investigators of that subject who had urged a distinction between local glacial phenomena and the general glaciation of our continents. It was because he was familiar with the distinction between these two sets of facts that he had always held, from the very beginning of his investigations, that there was a time when our earth presented climatic conditions so totally different from those now obtaining, that the northern hemisphere was covered by an extensive sheet of ice, and that the phenomena to be ascribed to the agency of that sea of ice moving from north southward were those uniform glacial appearances which we find over continental expanses, and traces of which we find even in high elevations. He had been convinced that whoever should explore the southern hemisphere on an extensive scale would find the evidence from extensive glaciation on the southern hemisphere as well as on the northern, but that the trend of the southern ice sheet and the transportation of bowlders would be reversed. Instead of moving from the north southward as in the northern hemisphere, the movement should be from the south northward, and the accumulations of loose materials in southern moraines should present an arch curving northward. He could say that he had seen in the southern hemi-

sphere all that he had expected to find. The occurrence of these phenomena on a large scale in the southern hemisphere tended at once to establish the fact that the glacial phenomena were cosmic phenomena, and were not owing to local geological occurrences. He contended that the ability to recognize glacial phenomena depended in a great measure upon thorough familiarity with it, there were so many elements to be taken into account. Yet the track of the glacier could be detected as certainly as the hunter detects the track of his game. Causes of deception in interpreting the glacial phenomena were pointed out in detail. He showed the distinction between local glacial phenomena and phenomena belonging to general glaciation. The evidence obtained from erratic bowlders was examined and apparent contradictions explained. In some of the New England regions he had traced the tracks of bowlders for seventy miles in unbroken continuity. In the southern hemisphere he had traced them over a much longer distance.

He would make a statement which he expected would not be accepted for many years; it was that all our mountains below eleven thousand feet had all been scored over by the great sea of ice; that the whole range of the Rocky Mountains had been under ice, with only a few prominent peaks, perhaps rising above the fields of ice. He thought that the great ice sheet could not have been less than ten or twelve thousand feet thick and might have been thicker. In the Andes he had become acquainted with signs of glacial action twelve thousand feet above the sea.

Prof. J. D. Whitney, State Geologist of California, read a paper on "Notice of Investigations making in California on the reliability of the barometer as a hypsometric instrument." His remarks were illustrated by charts and tables.

Prof. Agassiz and Prof. Hilgard followed in remarks commending the geological survey of California as a work of great national importance, and hoping that the Academy would use its influence to prevent its interruption.

Prof. A. E. Verrill gave an interesting account of results of dredging on the coast of New England in connection with the United States Fish Commission. The explorations had added at least three hundred and fifty species to the fauna. Among the polypes prior to this investigation there were known but twelve species. They had added seven species. They had added thirty-eight species to the forty-eight Acalephs; ten species to the Echin-

oderms; ninety-five to the mollusks, one hundred and twenty-five to the worms and ninety to the crustacea. Additions to the Echinoderms and others were mentioned.

The second day's session was concluded by remarks by Prof. Agassiz about "Scientific Views entertained upon Theoretical Grounds." Prof. Agassiz' remarks were a protest against hastily adopting scientific theories unsupported by sufficient matter-of-fact evidence. He felt more and more the danger of stretching inferences from a few observations. The manner in which the evolution theory in zoology is treated would lead those who are not zoologists to suppose that observations have been made by which it can be inferred that there is in nature such a thing as a gradual change among organized beings, and that the transformation has actually been traced. But there is no such record, and it is shifting the grounds from one field of observation to another to make such statements. When the assertions go so far as to exclude from the domain of science those who will not be dragged into the mire, he thought it time to protest.

On the concluding day of the session Mr. Alex. Agassiz spoke on the Affinities of Echinoderms and Worms, and Prof. Agassiz on the Reproductive Organs of the Selachians compared with one another and with those of the Vertebrates.

At a meeting of the Indianapolis Academy of Science, Prof. E. T. Cox exhibited a meteorite about four pounds in weight, found by Dr. Seville, in 1870, in the plastic clay under a bed of peat in Howard County, Indiana, about seven miles east of Kokomo.

Mr. G. R. Crotch is engaged in preparing a checklist of the Coleoptera of North America to facilitate exchanges and records of faunas. It will make a pamphlet of about 70 pages, to cost 50 cents, and will be published by the Naturalists' Agency. Subscriptions are requested that the size of the edition may be at once determined on.

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Mr. Edward Whymper has arrived at Copenhagen from his second exploration of W. Greenland. He brings with him rich collections of curiosities, and some singular specimens of fossil wood.

Professor Agassiz has recently been elected a foreign associate of the French Institute (Academy of Sciences). It may be remembered that the number of foreign associates of the Academy of Sciences is limited to eight.

We are glad to learn that Mr. Charles Stodder has saved from the conflagration of November 9th, all his valuable stock of Tolles's telescopes, microscopes, and microscopic objectives. Work in the shop will go on, and all orders filled as usual.

## ANSWERS TO CORRESPONDENTS.

J. M., Penn.—The glass paraboloid, "Wenham's Parabola," has been prominently brought forward as a means of transparent illumination with high powers, by Chevalier Huyttens de Cerbecq, of Brussels. As thus used it gives excellent definition with a well corrected lens, but fails completely with a poorly corrected one.—R. H. W.

R. M. D., New York.—It would probably be undesirable to have all objectives mounted in brass-work of exactly the same length measured from the focal point of the objective to the top of the mounting. The convenience attained in working with the straight form of a double-nose piece would not compensate for the disadvantage of wearing the rack almost entirely at one point. A general uniformity of length of compound body, however, is greatly to be desired.—R. H. W.

W. W. R., Indiana.—1. The best microscopical definition attainable at present is by means of "immersion" lenses. 2. They are practically as durable as dry lenses, since both will last, with frequent but careful use, until rendered obsolete by the improvements constantly being made in their manufacture. 3. They are not more liable than other objectives to injury by changes of temperature, etc. 4. Several first-class makers have been accustomed for many years to furnish immersion objectives which can be instantly transferred, by screw-collar adjustment, to dry objectives. Thus the owner has a choice of style, with no compensating disadvantage.—R. H. W.

## BOOKS RECEIVED.

*Verhandlungen der K. K. geologischen Reichsanstalt.* Nos. 1-6. 1872. 8vo.
*Jahrbuch der K. K. geologischen Reichsanstalt.* Bd. xxii. No. 1. Jan.-March, 1872. 8vo.
*Bericht ueber die Thatigkeit der St. Gallischen naturwissensch. Gesellschaft.* 1870-71. 8vo. 1872.
*Proceedings of the Royal Society of Edinburgh.* 1870-71. 8vo.
*Annales Academici.* 1867-68. Leyden, 1872. 4to.
*Sitzungsberichte der K. Akadamie der Wissenschaften. Math. Naturw. Classe.* Bd. lxiv. Abth. 1. Heft i-v. Abth. 2. Heft i-v. 8vo. Wien. 1871.
*Denkschriften der K. Akademie der Wissenschaften. Math. Naturw. Classe.* Bd. 31. Wien, 1872. 4to.
*Sitzungsberichte der Naturwissensch. Gesellschaft Isis in Dresden.* Jahrg. 1872. Jan.-March. Dresden, 1872. 8vo.
*Memoires de la Societe Royale des Antiquaires du Nord.* 1866-71. Copenhagen. 8vo.
*Transactions of the Literary and Historical Society of Quebec.* 1871, 1872. Quebec. 8vo.
*Proceedings of the Boston Society of Natural History.* Vol. 14, pages 225 to end. 8vo. 1872.
*Memoirs of the Boston Society of Natural History.* Vol. 2, part 2, No. 3; part 2, No. 4; and Part 2, No. 5. 4to. 1872.
*Proceedings of the Academy of Natural Sciences of Philadelphia.* Part 2. May and Sept., 1872. 8vo.
*A New Theory of the Origin of Species.* By B. G. Ferris. 12mo, pp. 69. New Haven, Chatfield and Co. 1872.
*On the Fossil Red Hematite Ore of Bedford Co., Penn.* By J. P. Kimball. 8vo, pp. 22. 1872.
*On the Silver Mines of Cusihuiriachic, Mexico.* By J. P. Kimball. 8vo, pp. 11 and map. 1872.
*Report on the Fairchauer Iron Company Estab.* By J. P. Kimball. 8vo, pp. 28. 1872.
*Le Naturaliste Canadien.* Quebec. Dec. 1872. *Petites Nouvelles Entomologiques.* Paris. No. 64. Dec., 1872.
*Feuille des Jeunes Naturalistes.* Paris. No. 29. Dec., 1872.